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OF

PSYCHOLOGY AND PHILOSOPHY

I.—THE THEORY OF CONCRETE UNIVERSALS (I.).

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It is very hard to discover what is meant by those philosophers who speak about concrete universals. In this paper I shall try to elucidate what Bradley and Bosanquet wrote about the theory of concrete universals, and to offer some criticisms of their argument.

Terminology.

A great many philosophers have held that the fact that a number of different objects are red or square can only be accounted for by holding that an identical quality, redness or squareness, is present in all the red or square objects. The singleness of the class of red or square objects is held to be due to the presence in the many individuals of an identical quality. Redness and squareness would then be called universals. The theory that there are universals like redness and squareness arose, then, to explain how a class can be both one and many. Those who deny that there are universals in this sense would maintain that the unity of a class can be accounted for without supposing that the individuals form a single class because a single universal informs each of them. Those who use the word "universal" in the sense of the theory just mentioned and of the attackers of it, mean by a universal something that would be *contrasted with* an individual

(or particular). They may hold very different views as to how, or whether, it is possible for individuals and universals to be related, but they are united in holding that individuals and universals are very different types of entity. It is also pointed out that the theory of universals should apply to relations between individuals, as well as to qualities or characters. Thus, if there are universals, there are universal relations as well as universal qualities.

Theories other than the theory of universals have been suggested to account for the unity of a class and the diversity of its members. Although these theories involve the denial of universals, or may even suggest that the theory of universals, because it is nonsense, cannot even significantly be denied, it may be said about all of them that they are attempts at solving the "problem of universals." The problem of universals, that is to say, is not necessarily concerned with universals at all, and only gets the name because the theory of universals was one of the earliest and most generally accepted theories to account for the fact that the same thing may be said about a great many different individuals.

There are differences, even between those who believe that there are universals, as to the exact nature of the contrast between universals and individuals. It is fairly generally agreed, however, that while individuals come to be and pass away in time, universals are timeless. This red object may cease to be red, or cease to exist, and another object may become red. But redness itself is independent of changes in the individuals in which it is manifested. Another way in which individuals and universals are contrasted is expressed by saying that universals are abstract, while individuals are concrete. Three things are meant, I think, by the expression that universals are abstract. It is meant, in the first place, that universals have a very different type of being from that of individuals. In the second place it is meant that, in order to be aware of universals, we have to think of several individuals and notice the respects in which they are alike. They are objects which we can only become aware of by exerting our faculty of thinking. Some philosophers who apply the adjective 'abstract' to universals also mean by it that universals can have no existence apart from the individuals in which they are manifested.

When individuals are being contrasted with universals they are sometimes called concrete. Here "concrete" may stand for the different sort of being that individuals are supposed to have in contrast with universals, and is the converse of the first sense of "abstract". Whether "concrete" is ever applied to

individuals to mean that, unlike universals, they are not apprehended by thinking and comparing, I do not know. It is quite possible. Some philosophers would say that individuals (or particulars) differ from universals in a different way from the way in which substances differ from attributes. Since substances are called concrete, in contrast with attributes, this would give us another sense of "concrete". According to the theory of universals there cannot be individuals which are not instances of universals, and so we cannot contrast "concrete" with the third sense of "abstract".

Another sense of "concrete" which it is necessary to mention here is the sense in which we speak of a "concrete proposal". Here "concrete" means "specific". In this sense of the word a specific shade of red would be concrete in comparison with redness or colour. In this sense, that is to say, a determinate is called concrete in comparison with the determinable of which it is a determinate. But in this sense of the word it is something that is not individual (or particular) that is concrete. For according to adherents of the theory of universals, even a specific quality, a specific shade of red, for example, is a universal, and therefore to be contrasted with its instances.¹

There are other senses of the words 'universal', 'individual' (or 'particular'), 'concrete' and 'abstract', but the need to mention them will not arise until later.

Bradley's Theory of Concrete Universals.

In the face of the discussions of the previous section, the phrase 'concrete universal' will appear strange. It is just possible that someone who realised the ambiguities of the word 'concrete' might nevertheless call a determinate universal quality or relation a concrete universal, in contrast with a determinable quality or relation. But he would have to realise that a universal could not be concrete in the sense in which an individual is. By 'concrete universals', however, idealists do not mean 'determinate universal qualities or relations'. It will be the business of this paper

¹ There are several ambiguities in my statement of "the theory of universals", but they must be allowed to stand for the time being. Thus, I have spoken indifferently of universals 'having instances', 'being manifested in individuals (or particulars)', and 'being present' in them. Again, I have spoken of 'individuals (or particulars)', not making it clear whether universals are held to be manifested in particular qualities (if there can be such), or in individual things. I only want to say enough about terminology, however, to be able to contrast the idealist theory with the more usual ones. I do not wish, here, to examine the more usual ones.

to show what they do mean by 'concrete universals', and why they believe that there are such entities. The most convenient way of doing this will be to indicate what arguments Bradley put forward in the first edition of his *Principles of Logic*, and then to show how they were developed and added to later, both by him and by Bosanquet.

In his *Principles of Logic* Bradley distinguishes abstract universals from concrete universals, and abstract particulars from concrete particulars, and goes on to say that 'concrete universals' and 'concrete particulars' are different names for individuals. Thus we have to explain, not only how the adjective 'concrete' can be applied to universals when a 'concrete universal' does not mean a 'determinate universal', but also in what sense the adjective 'abstract' can be applied to particulars. And we have also to explain how a universal, of any kind whatever, can be an individual too. Clearly a distinction is required between particulars and individuals.

Bradley first raises these questions in the long and important second chapter, where he is endeavouring to show that, if judgments were unions of ideas they could never be categorical. He is led on to ask what we mean by the reality about which judgments are judged, i.e., he asks what it is to be *real*. After suggesting various answers to this question he says :

But the simplest account, in which the others possibly are all summed up, is given in the words, The real is self-existent. And we may put this otherwise by saying, The real is what is individual.¹

In the immediately succeeding passages Bradley then goes on to make the following points. (a) By 'individuals' he does *not* mean simple, indivisible entities, which together make up the complex world. (b) By 'individuals' he means entities which, although they contain complexity and diversity, nevertheless remain single entities. (c) Since, therefore, individuals are entities which retain an identity amidst differences, they may properly be called universals. (d) Bradley gives the name 'particular' to something that *would*, if it existed, be simple, indivisible, and exclusive of other particulars. (e) He suggests that metaphysics can show that, in this sense, there *are* no particulars. (f) For what immediately appears to us always occupies some space or time and always exists in more spaces or times than one (since space and time are continuous). What thus appears is therefore identical in different times or different

¹ Bradley, *Principles of Logic*, Second Edition, p. 45. (Afterwards referred to as *P.L.*.)

spaces. Entities which are identical in differences are universals. Therefore what immediately appears to us is always a universal, and if what immediately appears to us is not particular, nothing else is likely to be.¹

Bradley next refers the reader to chapter vi of the *Principles of Logic*, on the quantity of judgments, where he discusses the distinctions between universals, individuals and particulars in greater detail. There he distinguishes the two senses of "universal" and the two of "particular" that have already been mentioned. By an *abstract universal* I think he means the sort of entity that we have called just a universal. He calls it 'abstract' in contrast with the *substantial* being of what it belongs to, i.e., 'abstract' is used in the third of the senses previously distinguished.²

The abstract universal, Bradley says,

is nothing whatever but an adjective. It is an epithet divorced, a shadow which apart from its body is nothing, and cannot exist.³

What Bradley here means by a *concrete universal* has already been shown. He means an individual, and his reason for holding that individuals are universal is that they are entities which preserve an identity amidst differences. The individual Julius Cæsar, for example, is the same individual although he is sometimes in Italy, sometimes in Gaul, sometimes plotting, sometimes sleeping, sometimes writing books, etc. The term "*abstract particular*" describes nothing that exists, for what exists is individual and thus, maintaining its identity amidst differences, is universal. Nothing exists that contains no differences along with its self-sameness. An abstract particular, if it existed *would be* a simple entity, identical with itself and exclusive of all others. But nothing is simple or (in this sense) atomic.⁴ The sense in which the adjective "abstract" is here used to qualify "particulars" is explained by Bradley as follows. It often happens that an inseparable aspect of something may be *considered* or thought of apart from the totality to which it belongs, but although this is possible it could not exist apart from its totality. Something

¹ Cp. Hegel, *Phänomenologie des Geistes*, A.I.

² The sense in which a universal is called abstract because it could not exist by itself, but requires a particular or individual in which to be instantiated or manifested.

³ Bradley, *P.L.*, p. 187.

⁴ It is argued by idealists that if there were external relations there would have to be simple elements. One sort of simple element, at any rate, would be entities such as are here called abstract particulars. Thus the theory that there could not be abstract particulars is regarded as an objection to the theory of external relations.

which is not only considered in isolation from a complex from which it is in fact inseparable, but is also mistakenly supposed to exist part from it, is called an "abstraction". Abstract universals are abstractions in this sense, for they could not exist apart from the individuals in which they are embodied, although it is sometimes supposed that they might. Bradley means that the same thing is true with abstract particulars. Individuals, which *do* exist, have their aspect of particularity or exclusiveness as against other individuals, and abstract particulars are this aspect wrongly considered as having a separate existence. We are now brought to *concrete particulars*. They are individuals considered as distinct from other individuals. Their being distinct from other individuals, however, does not make them particular in the sense in which abstract particulars would be particular. Concrete particulars are concrete universals regarded as distinct from other concrete universals. The adjective "concrete" is used to show that they are not abstractions, like abstract universals and abstract particulars. The phrases "concrete universal" and "concrete particular" stand for the same thing, but the latter phrase indicates that there may be a number of distinct concrete universals or individuals.

Bradley's theory then is that what exist or are real are individuals, and individuals, being identities in difference, are universals. So one name for individuals is 'concrete universals'. Individuals such as Julius Cæsar or this table are exclusive one of another, and we may therefore say that they have an aspect of particularity. From this point of view they are called concrete particulars. According to Bradley, if we suppose that the aspect of particularity may exist by itself, apart from the individual to which it belongs, we are falsely asserting the existence of abstract particulars. Abstract universals are what other philosophers have called just universals, and Bradley asserts that, like abstract particulars, they cannot exist "except in our heads".

Remarks on Bradley's Theory.

A number of points in the theory just described are in need of comment and discussion.

(i) It might be said that, quite apart from his application of the adjective 'abstract' to it, Bradley uses the word 'particular' in an unusual sense. Usually, it might be said, 'a particular' means an *instance* of a universal; the entity, for example, with regard to which I can say "This is blue", is an instance of blueness. Bradley, however, means by a particular something simple and exclusive of others. It is not difficult to see, I think, how he

hits upon exclusion of each other as a mark of particulars. For the same universal may have many instances, and the theory of universals requires that these instances shall be numerically distinct from each other. Presumably Bradley is referring to this consequence of the theory of universals when he says that particulars are exclusive of each other. I am not quite sure how Bradley came to hold that abstract particulars must be simple too. I suspect that in part he was thinking of the derivation of the word 'individual' and thus of its most literal meaning, and that in part he was already requiring something to contrast with the identity in difference (and thus complexity) of concrete universals.

(ii) Some important statements are made by Bradley in these passages without any attempt at justification. In particular we have to notice that, while arguments *are* given to show that abstract particulars could not exist, none are given to show that abstract universals could not, unless the statement that they are abstract, *i.e.* abstractions, is taken as an argument. Further, in some passages it is implied that they have an existence, but 'in our heads'. The following remarks seem justified. (a) Bradley himself, in the second edition, seems to have noticed that the bald statement that abstract universals cannot exist is in conflict with another view of his, that whatever is conceived must therefore have some existence. On page 195 of the second edition Bradley says: 'For everything conceivable has existence in some sense'. Now if this be admitted, Bradley's statement about the non-existence of abstract universals only amounts to saying that they are not objects of the same type as individuals, and very few philosophers would wish to deny this. Thus Bradley has to admit that abstract universals and abstract particulars too, since they are conceivable, are part of the furniture of the world. He ought next to have busied himself with the problem of *how* abstract universals and abstract particulars can enter into the constitution of the world. This problem arises more clearly in Bosanquet's writings. It is clear that idealists will allot to abstract universals and abstract particulars only a subordinate sphere. (b) There would appear to be some discrepancy between what Bradley says about abstract universals and what, in the chapters on judgment, he says about ideal contents. It is clear, from his descriptions, that in describing ideal contents and abstract universals Bradley was describing the same thing. The descriptions tally, and both ideal contents and abstract universals are held to be abstractions. For the purpose of the theory of judgment, however, it is essential that universals shall not be merely 'mental creations' or exist only 'in our heads.' For it is not at all

plausible to hold that judgment consists in the referring of some 'mental creation' of ours to reality. In the chapters on judgment he does not say that ideal contents are 'mental creations'; in the chapter on the quantity of judgments he says that abstract universals are. Since ideal contents and abstract universals are the same, either judgment is not what Bradley says it is, or abstract universals are not 'mental creations'. This, of course, is only a criticism of Bradley's consistency, and could not be regarded as seriously affecting the idealist position.

(iii) It is very important that we should be quite clear as to what Bradley is asserting when he says (a) that no particulars exist, and (b) that what does exist is always a concrete universal. Bradley's argument—if I understand it correctly—is that those who contend for the existence of particulars hold that if anything is particular, what is given in sense is. Anything that is given in sense, he goes on, must at any rate persist through *some* time, and thus be the same at different times. What remains identical in differences, however, is not a particular, but universal, and hence whatever is given in sense is universal. We have to notice first that the sense of 'particular' in which what is given in sense is held not to be particular, is the sense in which a particular is what Bradley calls 'an atom', *i.e.*, something completely simple and numerically distinct from all other particulars. That is, even if Bradley's argument be granted, it only follows that what he calls *abstract* particulars do not exist. A great many philosophers would hold that what is given in sense is not completely simple but is nevertheless particular. They would hold that the lasting through some time is not incompatible with being particular. What this other sense of 'particular' could be will be discussed later. It is also worth noticing that someone might argue that, although abstract particulars (in Bradley's sense) are not immediately presented, they are nevertheless required *theoretically*. To this Bradley would reply that there could not be a plurality of simple particulars, because they would have to be related, and their being related would be incompatible with their being simple.

Bradley, therefore, in denying the existence of abstract particulars is not denying the existence of sense data; and sense data, according to a great many philosophers, are particular existences. Indeed, according to Bradley, sense data would be universals, because they last some time (even though it be very brief), and some of them cover expanses too.¹ I think that at

¹ For the sake of argument we can ignore the possibility that there are *momentary* sense data that are also non-spatial.

this point it will be useful if we examine what likenesses there are between, say, blueness and a sense datum which would justify philosophers in saying that they are both universals, although universals of different sorts. This will necessitate our looking for what differences there are between them as well; and since the differences are more striking, although according to idealists they are not more important than, the likenesses, it is with the differences that we shall here be chiefly concerned. Let us take as an example of a sense datum a noise, which we will call S. We can suppose that S exists at various times over a short period and thus has phases which may be labelled t_1 , t_2 , and t_3 . That is, t_1 , t_2 , and t_3 are phases of S. As an example of an abstract universal *i.e.*, of what is ordinarily called just a universal, we can think of blueness, which we will symbolise by B. This, that and the other blue patch we can symbolise by p_1 , p_2 , and p_3 . What we wish to determine is what likenesses and unlikenesses there are between the relation of S to t_1 , t_2 , and t_3 , and the relation of B to p_1 , p_2 , and p_3 .

There are several ways in which the relation of S to t_1 , t_2 , and t_3 differs from the relation of B to p_1 , p_2 , and p_3 . (a) t_1 , t_2 , and t_3 , as phases of S, must be continuous with each other; p_1 , p_2 , and p_3 need not be, and even if they are, there almost certainly exists a p_4 or a p_5 which is not continuous with them. (b) t_1 , t_2 , and t_3 could not co-exist. p_1 , p_2 , and p_3 can and do. If I had taken as my example of a sense datum a momentary spatial one, then the parts of it (it would not have phases), would be co-existent. So we shall have to say that *some* sense data have differences which could not be co-existent, while *no* universals have instances which could not be co-existent. (c) If the phase t_3 did not exist, S would be a different S. We can suppose p_3 as not existing, however, without having to regard B as a different B. It would be argued, however, that if S had only one phase, and B had only one instance, the disappearance of the phase in the one case and the instance in the other, would involve the going out of existence of the sense datum and the universal. (d) We can now see, in greater detail, how the non-existence of t_3 would entail S's being different. In the first place, something that did not contain t_3 would not be S at all. Thus, if S is to exist, t_3 must exist. As the various phases of S, t_1 , etc., pass away, there is a sense in which S is being diminished.¹ This

¹ Many philosophers would say that as S lasts it is added to—that as it continues to exist it is *increased*. If this is so, however, there is still a contrast between S and B. For B is not increased when a new instance of it comes into existence.

becomes clearer in the case in which S is a spatial sense datum. If various parts of it are removed, S becomes smaller. Even in the case of the non-spatial sense datum, there seems to be sense in saying that S is *composed* of t_1 , t_2 , and t_3 . There is little analogous to all this in the case of B. B could be B without having the instance p_3 . If p_1 and p_2 pass out of existence, there is no sense in which we can say that B is being diminished, but only that the number of its instances is being diminished. Nor can we think of any universal such that p_1 , p_2 , etc., could be *parts* of it. B is not composed of p_1 , p_2 , and p_3 . (e) S lasts through a time. It is doubtful whether there can be instantaneous sense data. Many philosophers would certainly say that B does not last through a time, although p_1 , etc., do. That is, they would say that while there is sense in saying that this blue patch lasts or endures, there is no sense in saying that blueness lasts. (f) t_1 , etc., are not *instances* of S, while p_1 , etc., are instances of B. Instances of a universal do not compose it. (g) There may be other entities *like* S. Since B is relatively determinate, there may be other entities like B, as, for instance, violet or green. If, however, B had been a determinable, like colour, there could have been nothing else like it.

We must now notice two likenesses that might be alleged to exist between S and B. It is said by idealists that t_1 , t_2 , and t_3 are differences of S, and p_1 , p_2 , and p_3 are differences of B. Now it is clear, in the first place, that t_1 is different *from* S, and p_1 different from B, and so on with t_2 and p_2 , etc. But 'of', I suppose, is used instead of 'from' to show that there is very intimate connection between S and t_1 , and B and p_1 . The intimate connection is that S could not exist without t_1 , t_2 , and t_3 —for they are parts of it, and a whole cannot exist without its parts. It might also be argued that blueness cannot exist without blue patches. But although there is an intimate connection in each case, it is a connection of a different sort. S would not be S unless composed of t_1 , t_2 , and t_3 . B would still be B so long as, say, only p_3 existed. The relation of t_3 to S is that of part to whole, the relation of p_3 to B is that of instance to its universal. So if we call them both differences, we must remember that they are different sorts of difference. It might also be said that there is a likeness between t_1 , t_2 , and t_3 and p_1 , p_2 , and p_3 , in that both sets of entities are classes. This is so, but I do not think it is a very important likeness. For although these two sets are alike in being classes, S is related to the class t_1 , t_2 , and t_3 in a different way from the way in which B is related to the class p_1 , p_2 , and p_3 . B is the defining characteristic of the one class. S is not the

defining characteristic of the other; the defining characteristic of the other is 'being a phase of S'.

(iv) Idealists have more to say on identity in difference than I have touched upon yet. Bosanquet in particular argues, that not only are individuals and abstract universals identities in difference, but that individuals are universals in a more primary sense than abstract universals are. He thinks that, although abstract universals are identities in difference, they show a falling off from the 'true type' of the universal. Our next task, therefore, will be to state and discuss what Bosanquet and Bradley between them have to say about identity in difference.

Identity in Difference and the Principle of Identity.

I do not think it is feasible to begin by stating what the doctrine of identity in difference is, and then to show what arguments have been put forward in defence of it. I think, on the contrary, that we shall have to approach the theory gradually, by seeing what views on identity and difference Bradley and Bosanquet attacked, and the various positions they came to while prosecuting these attacks. In its earliest stages their argument is concerned with two contentions. (i) They criticise the Principle of Identity as it is stated in most text-books of traditional logic. (ii) They argue that there can be no identity without difference, and also no difference without identity.

(i) The Principle of Identity, along with the Principles of Contradiction and Excluded Middle, used generally to be formulated and discussed in a chapter entitled 'The Laws of Thought', and it was frequently symbolised by 'A is A'. This was often taken to mean 'Anything is itself', and the best-known statement of the principle is, perhaps, Bishop Butler's phrase: 'Everything is what it is and not another thing'. Both Bradley and Bosanquet, however, are quite clear that 'A is A' is a formula capable of several interpretations. In two possible senses they accept it, but in what they take to be a very important and natural sense they reject it. I need only deal with the sense in which they reject it.

Their objection is that the formula 'A is A' is not suitable for expressing any *judgment* at all. Judging is the asserting of something, and where there is no difference between subject and predicate, nothing is asserted, and so there is, properly speaking, no judgment. Since the formula 'A is A' uses the same symbol for the predicate as it does for the subject, it fails to bring out the essential of judgment. The formula 'A is B' would be a

better means of symbolising judgments, although not a good one either. It would be better in so far as the *difference* between subject and predicate would be shown. It would not be a good means, however, because A and B might stand for *any* subject and *any* predicate, and so nonsense like 'London Bridge is one o'clock' would also be symbolised by 'A is B'. The objection, then, to 'A is A' as expressing the Principle of Identity is that it is not fitted for expressing judgments at all, but only tautologies. If, therefore, 'A is A' were true, there would be no judgments. If the Principle of Identity is a principle of tautology, it is not itself a judgment nor, if it is universally applicable, are there any judgments.

(ii) What, then, do idealists say in favour of the view that there is no identity without difference, and no difference without identity? The most important argument used to show this, is to the effect that identity without difference (which is called *abstract* identity), would make judgment impossible.¹ If identity were abstract (in this sense) there would be tautologies but no judgments, for in the judgment 'A is B', there would be no difference between A and B, and so nothing would be asserted. If, then, judgment *is* possible, there must be some *other* sort of identity, which is called identity in difference or *concrete* identity. A consideration of Bosanquet's example, $7 + 5 = 12$, may throw some light upon what is meant by 'concrete identity'. If $7 + 5$, he says, were not identical with 12, the judgment, clearly, would not be true. Equally clearly, it cannot be *merely* identical with 12, for if it were, there would be no difference between the judgment $7 + 5 = 12$ and the tautology $12 = 12$. Nor, on the other hand, can $7 + 5$ be merely different from 12, for if it were, there would be no judgment to the effect that $7 + 5 = 12$. Idealists, therefore, adopt the doctrine of identity in difference as a method of escape from this dilemma. $7 + 5$ is both itself and other than itself. If *anything* were just itself no judgments could be made about it. It is therefore just wrong to say that 'everything is what it is and not another thing'. If it were, nothing could be said about anything. Bosanquet alleges that a result of denying identity in difference (or concrete identity), and of accepting in consequence the Principle of Identity, in its false interpretation, is an extreme philosophical atomism or pluralism. For then each entity in the world is regarded as merely self-identical and isolated from the rest.

¹ Bosanquet, *Science and Philosophy*, pp. 35-36. Bosanquet, *The Meeting of Extremes*, etc., pp. 103-104.

The reason why, according to idealists, there can be no difference without identity, is that difference is a relation, and no terms can be related unless they are identical in some respect.¹

Remarks on the Preceding Arguments.

A number of observations upon the preceding arguments seem to be necessary.

(i) The idealists argued that, in the sense in which the Principle of Identity is a tautology, it is not itself a judgment at all. Some other philosophers would be inclined to agree with this, inasmuch as they say that *none* of the laws of logic are judgments (or propositions) in the sense in which 'This is soft' or 'Crows are black' are judgments. These philosophers would say that it is their being tautologies which distinguishes the propositions of logic from other propositions. They would not, therefore, regard it as a reproach to the Principle of Identity, that it is a tautology. Idealists, however, argue that being a tautology is incompatible with being a judgment in *any sense*. Tautologies, they say, are nonsense. Their reason is that in a judgment the subject and predicate must be distinguishable, because if they are not, nothing is asserted about the subject. In tautologies, however, the subject and predicate are identical, and so nothing is asserted. Where nothing is asserted there is no judgment of any sort. It will be seen that this argument depends upon the assumption that all judgments have a subject and a predicate. The idealists' conclusion, I think, *would* follow if this assumption were granted, because propositions of the subject-predicate form cannot be tautologies. 'Mortality is mortal', or 'Socrates is characterised by Socrates' are both nonsense.² We can only, therefore, avoid the idealists' conclusion by pointing out that, although both may be expressed by 'is', the relation³ of identity is different from the relation between subject and predicate. 'A is A', therefore, would be nonsense if in it 'is' stood for the relation between subject and predicate, but, it may not be nonsense if 'is' stands for identity, or for some other relation.

¹ Bradley, *P.L.*, Vol. 2, p. 651. '... diversity apart from identity has lost its sense.'

² It might be said that what Kant called analytic judgments are tautologies and yet have the subject-predicate form. But what Kant called analytic judgments are in fact judgments of the form: 'if *a* has the predicates *r* and *s*, it has the predicate *r*'. This is not a judgment of the subject-predicate form.

³ I use the word 'relation' here for lack of some other.

(ii) It is true that the idealists could question the legitimacy of this distinction. They could say that those who make it are committed to the mistaken view that there are fixed, ultimate, and utterly distinct types of judgment. In the judgments that are actually made, they would say, types are always mingled, and it is only abstractions which can be of one type exclusive of all others. This is one of the main contentions of Bosanquet's *Logic*.

(iii) Besides saying that the Principle of Identity is itself not a judgment, idealists go on to say that if, in its obnoxious sense, it is true, it implies that *all* judgments are tautologies. For this contention to have any substance they must interpret 'A is A' to mean: 'In every judgment the predicate is identical with the subject'. We have just seen that in *no* judgment is the predicate identical with the subject, and that it is plausible to hold that there are types of judgment other than the subject-predicate sort. We need not, therefore, linger over this interpretation, which it is hard to believe that anyone has seriously entertained. It is obvious that in 'A is A' the 'A's' might stand for different sorts of entity. They might, for example, stand for individuals, or classes, or propositions, and so the Principle of Identity might be several principles: 'Any individual is identical with itself', 'Any class is identical with itself', and 'Any proposition is identical with itself'. It is easy to see that other interpretations are possible. It would not follow from any of these interpretations of the Principle of Identity, or even from a more general statement of which they are special cases, that *all* judgments are tautologies in the sense that there is no difference between their subjects and predicates. A non-tautologous judgment, for example, must be identical with itself, and it is only the judgment which expresses this that is tautological. From the fact that the Principle of Identity is tautological it does not follow that all judgments are. And if we accept the distinction between the relation of identity and the relation between subject and predicate, we can believe, both that an individual is identical with itself, and that it has predicates which *cannot* be identical with it.

(iv) We must now examine the contention that there can be no identity without difference. As Mr. W. E. Johnson has shown,¹ there are senses in which this contention has to be admitted. When we say, for example, that a certain word has the same meaning in different contexts, we are saying that the word has an identical meaning, although the contexts are different. It should

¹ W. E. Johnson, *Logic*, Vol. 1, p. 186 ff.

be noted, however, that it is one thing that is identical and *others* that are different. It is not the same thing that is both identical and different. Similarly, we may become aware of the same thing in different acts of thought. Also, when we define words or phrases, we do so in propositions of the form: '*x* is identical with *y*', where *x* and *y* stand for different words or phrases, *i.e.*, '*what x means is identical with what y means*'. Here we have otherness of phrase, but identity of meaning. In all these cases, in order to refer to an identity, it is necessary to refer to differences as well. But the things or meanings that are identical are not *also* different, they are what is identical in different contexts, situations, etc. With such instances in mind, Mr. Johnson concludes:

thus the statement that identity implies difference is correct in the sense that the asserting of identity between one pair of terms implies our having implicitly or tacitly asserted difference between another pair of terms.¹

We have to add, however, *another* sense of identity, which arises when we say, for example, that Joan of Arc was an identical person (and not two, one of which was the double of the other), or that we climbed that identical tree. This is the identity which we attribute to things or continuants, in respect of which they *remain the same*. In order that it may remain the same thing, a continuant must pass through phases, and so, in order to be identical it must possess differences. Here 'differences' means the 'phases' or 'events', or whatever else is supposed to make up the continuant. This also, then, might be meant by the statement that there can be no identity without difference. But although these are some of the things that *might* be meant by it, idealists do not mean them. Idealists believe literally that nothing can be identical with itself, unless it is also *different from itself*. Bosanquet calls this "the paradox at once of reality and of inference".² It is upon this contention that the theory of concrete universals rests. Before we discuss this, however, it is necessary to show the connection between the possibility of inference and the existence of identities in difference or concrete universals.

¹ W. E. Johnson, *Logic*, Vol. 1, p. 188.

² Bosanquet, *The Meeting of Extremes in Contemporary Philosophy*, p. 112.

(To be concluded.)

II.—MR. WISDOM ON PHILOSOPHICAL ANALYSIS.

BY A. H. S. COOMBE-TENNANT.

[*Note on the use of brackets and inverted commas in this Paper.*

Apart from their ordinary use, brackets and inverted commas have a special use in this Paper in connexion with sentences and facts, illustrated by the following examples.

(a) *Brackets and inverted commas enclosing sentences.*

For (A loves B) read The fact that A loves B.

For "A loves B" read The sentence "A loves B".

(b) *Brackets and commas enclosing single words.*

For (England), (red), (between), read England (as opposed to the word "England"), the quality Red, the relation Between.

For "England", "red", "between", read The word "England", the word "red", the word "between".]

I. INTRODUCTORY. In this Paper I propose to expound and criticise Mr. John Wisdom's theory of the nature of philosophical analysis, as set out in five articles on "Logical Constructions" in *MIND* 1931-33, an article entitled "Ostentation" in *Psyche*, vol. xiii., 1933, and a Paper read to the Aristotelian Society in 1934 in a symposium on the question "Is Analysis a useful method in Philosophy?"

Those who have read these articles will remember that the development of this theory is incidental to and consequent upon a close and detailed study of the expressive function of language. I shall try to explain those conclusions of this study that are necessary for the understanding of the theory that follows, but I am not going to attempt a critical survey of the study itself.

The remainder of this Paper falls into two sections, (1) Expository, in which I try to give an accurate simplified account of Wisdom's theory, and (2) Critical, in which I consider how far it can be accepted, and briefly put forward my own views on the nature of philosophy and philosophic method.

II. EXPOSITORY.¹ The following is a general statement of Wisdom's theory. The business of philosophers is to discover the ultimate structure of facts. Their object should be to gain clearer insight into the structure of the facts they already know, rather than to attempt to use these facts as a basis from which new facts may be deduced. To gain this clearer insight is to perform a process of philosophical analysis that finds its expression in what he calls Ostentation. Ostentation means replacing the sentences we ordinarily use to express the facts we know—sentences that seldom attempt to display the ultimate structure of these facts—by sentences that do display their ultimate structure. This is the outline of Wisdom's view, expressed in his own technical terms: we must now consider in detail what it all means.

We will start with his view of the relation between sentences and facts, which is developed in the articles on Logical Constructions. Let us begin by considering how Wisdom uses the words "sentence" and "fact". By a *fact*, Wisdom means just what everyone else means. We all understand what it means to face facts, to argue from them, to point them out, to think of a hypothesis to cover them, etc., etc. (*E.g.* "You have got to face the fact that he has been dead seven hours, and your hypothesis does not allow for that fact.") Wisdom's use of the terms "element", "constituent" and "component" in connexion with facts requires explanation. (A loves B) has three elements, (A), (B), and (loves). Similarly (A is between B and C) has four elements, and (A is red) two. The elements of a fact are either constituents or components. The functions of the two differ. Components are qualities or relations predicated of constituents: (loves) is component in (A loves B); (red) is component in (A is red). The constituents of a fact are the elements of which the component is predicated: (A) and (B) are constituents in (A loves B). Qualities and relations can function as constituents, for example in such facts as (Red is darker than pink), where the qualities (red) and (pink) are constituents related by the component (darker than), or (Between is a triadic relation) where being triadic is predicated of the relation (between).

There are people who ask Are there such things as facts? Is there the fact that I am happy as well as my happiness? According to Wisdom this is a silly question. To speak of facts is just

¹ I do not guarantee that this exposition is free from inaccuracies and misunderstandings, but it is the less liable to them on account of my having had the privilege of discussing with Wisdom the points raised. He has kindly looked through what follows and has pointed out some inaccuracies which have been removed.

one way of talking about the universe. It may be better to talk about it in terms of events, just as it may be better to talk in terms of processes rather than in terms of things—a suggestion that Prof. Broad has rather misleadingly expressed by “Can things be dispensed with in favour of processes?” (Examination of M^r Taggart’s *Philosophy*, vol. i., p. 156.) But to ask Are there such things as facts? shows a complete misunderstanding of the way in which the term is used.

So much for Wisdom’s facts and fact-jargon. Now for sentences. By “sentence” Wisdom means “token-sentence”, not “type-sentence”. If I write “This is red” fifty times I have written the same type-sentence fifty times, but I have written fifty different token-sentences, and therefore fifty different sentences in Wisdom’s sense. In other words, a sentence in Wisdom’s sense is a particular occurrence—a sequence of noises, or of marks on paper. Now, whenever you can talk of a sentence in this sense you can talk of the *fact* that certain noises or marks are arranged in a certain way: for instance, if I write “This is red”, then there is the fact that the mark “is” is between the mark “This” and the mark “red”. When Wisdom talks of the sentence “ArB” he is always referring to the fact whose constituents are the marks “A”, “r” and “B”, and whose component is (between). Thus a sentence in Wisdom’s sense is a fact—of a special kind.

I said that it is the relation between sentences and facts (*i.e.* between facts of a special kind and other facts not of this kind) with which the articles on Logical Constructions are mainly concerned. *The relation . . . which relation?* And what is meant by saying that sentences are facts of a special kind? Wisdom says that philosophical analysis consists in replacing the sentences that we use to express the facts we know by sentences that express the same facts but in addition display their ultimate structure, which no ordinary sentence attempts to do. This tells us which relation it is between sentences and facts that Wisdom is concerned with: it is the relation that holds between a sentence and the fact or set of facts that it *expresses* or *is about*. The sentence “John loves Mary” expresses the fact that John loves Mary; the sentence “John is in love” imperfectly or incompletely expresses the same fact. Both sentences are about the fact that John loves Mary in a sense in which the sentence “John loves Mary” is not about the fact that Mary hates Ann, or that the richest man in Iceland drinks madeira with caviare.

It is this relation of expressing that we have to consider, and in particular a special form of expressing which Wisdom calls

sketching. Indicative sentences (we are not going to consider interrogative or imperative sentences) are facts of a peculiar kind: they are facts used by human beings to express other facts. In his contribution to the Aristotelian Symposium on the method of analysis in philosophy Wisdom writes as follows: "When we utter a sentence we do so because we wish to point out a fact. In ordinary languages it is understood that certain internal features of the sentence—its form, the words it contains, and the arrangement of these words—intimate the internal features of the fact; in particular it is understood that the form of the sentence intimates the form of the fact, the elements or words in the sentence the elements in the fact, and the arrangement of the elements in the sentence the arrangement of the elements in the fact". If we turn to the articles on Logical Constructions we find the same thing stated at enormous length, with a flourish of technical terms that makes it hard to see the wood for the trees. The following is, I think, a reasonably faithful simplified account of this portion of the articles.

Every fact has a *structure*. When you know what the elements of a fact are, and how they are arranged, you know its structure. The *form* of a fact is determined solely by the number of elements in it; two facts with the same number of elements are said to have the same form. Now what has just been said applies, of course, also to those facts that are sentences, whose elements are marks or noises. Suppose we express the fact that the poker is next to the shovel by using the sentence "This adjoins that". We then have, in addition to the fact that the poker is next to the shovel, another fact, whose constituents are the marks "This" "adjoins" and "that", and whose component is (between), in which the mark "This" is used to name the poker, the mark "that" to name the shovel, and the mark "adjoins" to name the relation (adjoins) that holds between the poker and the shovel. Now the two facts are not identical in form. The first has three elements, the poker, the shovel, and the relation (adjoins), but the second has three marks as constituents and the relation (between) as component—making four elements in all. If, instead of "This adjoins that" we had written simply "This that", we should have had a sentence of the same form as the fact that the poker adjoins the shovel, for this sentence would have only three elements—the marks "This" and "that" and the component (adjoins). "This adjoins that" names the relation (adjoins) that acts as component in the fact that the poker adjoins the shovel by the word "adjoins". "This that" does not name the relation (adjoins) by a specific mark; it shows it by

the way the two words are arranged, just as it is shown by the way the two fire-irons are arranged. The sentence "This that" is a kind of picture of the fact about the poker and the shovel, so long as "This" and "that" are used as names for the poker and shovel respectively. If we were to employ a more primitive language we could replace the marks "This" and "that" in the sentence "This that" by hieroglyphics—little drawings of the poker and the shovel. This sentence would literally be a picture of the fact it expressed.

Now, although "This adjoins that" does not mirror the structure of the fact it expresses in quite the same way as "This that", nevertheless both sentences have this in common, that they name the elements of the fact they express and show how they are arranged. Both sentences are faithful accounts of the fact in this respect. Now what Wisdom really means by "sketching" is this: that a sentence is a sketch of a fact if it names the elements of that fact and shows how they are arranged. In other words, a sketch displays structure. Unfortunately Wisdom attempts a precise definition of "sketching" which excludes a very large class of sentences that ought to count as sketches. For he lays it down that to be a sketch a sentence must either be identical in form with the fact it expresses, or with that fact's First Derivative. [The First Derivative of (ArB) is (A is related by r to B). The F.D. of a fact has one more element than the fact itself.] "This adjoins that" passes this test, since it is identical in form with the four-termed fact ((Adjoins) relates (this) to (that)), which is the F.D. of (This adjoins that). But owing to the structure of the English language many sentences that are really sketches fail to pass the test through the introduction of strictly superfluous words. For instance, "This is next to that" is just as good a sketch to anyone who understands English as "This adjoins that"; but it has too many elements to be identical in form with (This adjoins that) or its First Derivative, and thus fails to satisfy the definition. It is obvious that we can define a sketch as a sentence that displays the structure of the fact it expresses and at the same time recognise that many sentences express facts without sketching them. To this class belong sentences that contain descriptive phrases instead of proper names for the constituents of the fact they express.

We can now understand why that relation between a sentence and a fact which Wisdom calls sketching is of special importance in his philosophy. Since, on his view, the philosopher's concern is with the structure of facts, and since only sketches display

structure, it is obvious that sentences which are sketches are *philosophically* important, and those that are not are *philosophically* defective. But we must go further. Ostentation, according to the definition previously given, means replacing the sentences we ordinarily use to express the facts we know—sentences that hardly ever attempt to display the ultimate structure of these facts—by sentences that do display their ultimate structure. Structure is defined as what the elements of a fact are and how they are arranged; and we have seen that a sketch displays structure because it names the elements of the fact it expresses, and shows their arrangement. Is Ostentation, then, equivalent to sketching? No, for a sketch need only display structure, but an Ostentation must, by definition, display *ultimate* structure.

Now in order to understand Wisdom's philosophy it is extremely important to grasp the distinction between structure and ultimate structure. (It will presently appear that this is a genuine distinction misleadingly described, but this point must be left to emerge in the course of the argument.) Just as structure is defined in terms of elements and their arrangement, so is ultimate structure defined in terms of ultimate elements and their arrangement. Take the sentence "England invaded France", and suppose it expresses a fact. Then "England invaded France" is a sketch of a fact with (England) (invaded) and (France) as elements. But it is obvious that England and France are not the *ultimate* elements of (England invaded France). It is difficult to say what the absolutely ultimate elements are, but plainly we can point to *more ultimate* elements in the shape of the British soldiers who crossed the French frontier, and if we replace the sentence "England invaded France" by a more complicated sentence about individual Englishmen we shall have gone one step towards providing a sentence that displays the ultimate structure of (England invaded France). In order to see the exact sense in which Englishmen are said to be more ultimate than England it is necessary to introduce the notion of a series of levels of discourse. The conception we are trying to locate can be described either in terms of degrees of ultimacy of levels of discourse, or in terms of degrees of ultimacy of elements of facts. We shall have to use both ways of talking to begin with.

If we compare the substitution of "individual-sentences" for "nation-sentences" with the substitution, for a sentence in which the word "awe" occurs, of a sentence containing the words "fear" and "admiration", we shall see by contrast what is meant by "ultimacy of elements" and the expressions "same level of discourse", "more (less) ultimate level of discourse".

Suppose we take the sentence "Awe of a master produces servility" and substitute for it "Fear and admiration of a master produce servility". Now this is an analysis which resembles in an obvious way the analysis of "England invaded France" in terms of individual Englishmen and Frenchmen. In each case we have substituted a more for a less detailed statement. But there is an important difference which we can express in two ways: (1) by saying that to talk of fear and admiration is to talk at the same level of discourse as to talk of awe, while to talk of Tom, Dick and Harry is to talk at a different level of discourse than to talk of England; (2) by saying that fear and admiration are factual elements of the same order of ultimacy as awe, while Tom, Dick and Harry are factual elements of a different order of ultimacy from England. We say that to talk of fear and admiration is to talk at the same level of discourse as to talk of awe because to say something about awe is to say *the same thing* about a mixture of fear and admiration. We say that to talk of Tom and Dick is to talk at a different level of discourse than to talk of England because to say something about England is to say something, *but not the same thing*, about individual Englishmen. When we say that fear and admiration produce servility we mean that they produce it in exactly the same sense of "produce" as that in which we say that awe produces it; but we do *not* mean—or if we do we are talking nonsense—that Tom, Dick and Harry invade France in the same sense of invade as that in which we say that England invades France.

Perhaps the point will become clearer if we state it in terms of ultimacy of elements. Fear and admiration are elements (of any fact about them) of the same order of ultimacy as awe. Why? Because they are emotions in just the sense in which awe is an emotion; what you do to awe you do to them. When you feel awe you feel (same sense of 'feel') fear and admiration; when you repress awe you repress fear and admiration (or one of them), and so on. Tom, Dick and Harry are elements (of any fact about the English nation) of a different order of ultimacy from England herself. Why? Because England is not a thing in the sense in which Tom is a thing or Dick is a thing; you cannot do to England what you can to Englishmen. If you happen to be Bulldog Drummond you can pick Englishmen up and throw them about the room; but it would be nonsense to talk of literally touching or laying hands on England. Of course you could say that a man loves his friend and also that he loves England; but you would be using "loves" in two different senses: to say that a man loved England and his friend would

be like saying that he took his hat and his leave. England, then, is less ultimate than her nationals—much in the same way as a class is less ultimate than its members. Tom, Dick and Harry are more ultimate than England, because statements about England have to be analysed in terms of individual Englishmen.

When A and B are so related that (1) to say something about A is to say something, but not the same thing, about B, and (2) B is more ultimate than A in the sense defined, then A is said to be a *logical construction out of B*. Thus England is a logical construction out of Englishmen, and the working class is a logical construction out of working men. Similarly the Self is quite possibly a logical construction out of a certain group of inter-related sense-data and experiences. In this case not Tom, Dick or Harry, but their sense-data and experiences, are the ultimate elements of the fact (England invaded France); for to say that Tom crossed a frontier will be to say something of his sense-data and experiences—not of course that *they* crossed a frontier, but that they include such-and-such sense-data in a certain sequence, or something of the sort.

We can now begin to understand the distinction we have described as that between structure and ultimate structure. Unfortunately we are not out of the wood yet, for we are dealing here with a case where, in order precisely to locate a conception, it is necessary to describe it in two different sets of terms, and then to point out that one of these ways of talking is misleading.

The "ultimate-element" terminology suggests that the elements of a fact can be grouped in orders or layers of ultimacy, and that the process of ostensive analysis consists in stripping away one by one the less ultimate layers, till at last there remains only the absolutely ultimate layer with its absolutely ultimate elements. Now this account of ostensive analysis is highly misleading. It is misleading to suggest that (England invaded France) has England as an element *and also* the more ultimate elements Tom, Dick and Harry, *and finally* the rock-bottom elements which are Tom's, Dick's and Harry's sense-data and experiences; since if England is a logical construction out of Englishmen who in turn are logical constructions out of sense-data there is obviously a sense in which these sense-data are *the* elements in a fact about England, and in which England and Tom and Dick and Harry are *not* elements in it at all. On the other hand, there is plainly some sense in which England is an element in a fact about England, and some sense in which Tom & Co. are elements in that fact. Again, take the fact (England invaded France). In a certain sense this is a different fact from

the next-level fact about individual Englishmen and Frenchmen. It is different because it only has three elements, while the fact about Tom and Alphonse and the rest of them has a great many more. But in another sense it is the same fact, since to say that Tom and his pals crossed the frontier of the land inhabited by Alphonse and his pals is just to say that England invaded France. Similarly, in a sense, the ultimate fact about certain inter-related sense-data and experiences is yet a third fact; but in another sense it is the same fact differently stated. One is reminded of the paradox that figures so prominently in the Athanasian Creed. It is possible to remove the paradox by making some adjustments in the terminology which suggests it; this involves recognising a systematic ambiguity in the whole formal vocabulary, whereby words like "element" and "fact" are made to bear a series of slightly different senses corresponding to the series of levels of ultimacy of factual elements. "The fact (England invaded France) is the fact (Tom & Co. crossed the frontier of the land inhabited by Alphonse & Co.)" only expresses a fact if "fact", which appears twice in it, is being used in two slightly different senses. There must be a usage of "fact" appropriate to each level of ultimacy, and to that level alone. The words "sketch" and "element" will be subject to the same systematic ambiguity. Denoting these different usages by small numbers, we can say that the sentence "England invaded France" is a sketch₁ of the fact₁ (England invaded France). We can also say that the fact₁ (England invaded France) is the fact₂ (Tom, Dick and Harry, etc. . . .). We must not say that "England invaded France" is a sketch₁ of the fact₂ (Tom, Dick, Harry, etc. . . .); it isn't. What we can say is that "England invaded France" is a rough sketch₂ of the fact₂ (Tom, Dick and Harry etc. . . .), and a still rougher sketch (ultimate sense) of the fact (ultimate sense) that certain sense-data, etc. . . .

Now this usage, though it might be made to work, is clumsy and unsatisfactory. It is better therefore to drop altogether the ultimate-element terminology, and to talk instead in terms of levels of discourse. Using this terminology, we can describe the real position as follows. A Fact¹ F can be expressed by a certain number of sentences S₁ S₂ . . . S_n each of which is at a different level of discourse (in the sense previously defined). The Elements¹ of a Fact are what are described in the other terminology as the ultimate elements, or the elements (ultimate sense), and they are only named by the sentences S_n at the ultimate level of discourse.

¹ The capital denotes a neutral sense of the term, as distinct from any of the series in which it is used in the "systematic ambiguity" terminology.

Such a sentence is a Sketch¹ of F. *E.g.* (England invaded France) has for Elements (say) the sense-data and experiences of certain Englishmen and Frenchmen. It can be expressed at a certain level of discourse by "England invaded France", at another level by "Tom Dick and Harry crossed the frontier of the land inhabited by Alphonse & Co": both these sentences will express it, but neither will Name its Elements, and neither therefore is a Sketch. At the ultimate level of discourse we have a sentence of the form "Such-and-such sense-data and experiences were inter-related in such-and-such a way", and this sentence will name the Elements of F and show how they are arranged. It will therefore reveal Structure and be a Sketch.

This is the terminology finally approved by Wisdom as I understand it. Care should be taken to distinguish the use of "Element" "Sketch" "Fact" "Structure" etc. from the use of these words spelt without capitals. This latter use is such that it is correct to say that England is an element in the fact (England invaded France), and that "England invaded France" is a sketch of that fact. But "Element", "Fact" and "Sketch" are so used that it is incorrect to say that England is an Element, incorrect to say that "England invaded France" is a Sketch, and misleading to say that (England invaded France) is a Fact. The element of truth contained in the assertion that (England invaded France) is a Fact is that there is a Fact that is expressed by the sentence "England invaded France". And it is misleading to say that (England invaded France) is a Fact because there is no Fact *Sketched* by the sentence "England invaded France". Similarly, the element of truth contained in the assertion that England is an Element in the Fact expressed by "England invaded France" is that at a certain level of discourse the only correct way of expressing this Fact is by a sentence containing the word "England" (or its equivalent in another language). But it is incorrect to say that England is an element in the fact expressed by "England invaded France", because there is no element in that Fact of which "England" is the Name, *i.e.* because "England invaded France" is not a Sketch—although it expresses its Fact it does not Name its Elements, which are not nations but sense-data, and can only be Named by a sentence belonging to a more ultimate level of discourse.

Now, at last, with this terminology we can explain in the simplest way exactly what is meant by Ostentation. Ostentation consists in giving Sketches of Facts, *i.e.*, in substituting Sketches for sketches so as to reveal the Structure of Facts—their Elements

¹ See note on p. 440.

and Arrangement. To talk Ostensively is to talk at the ultimate or first level of discourse, to talk at any other level is to express without Sketching. To perform Ostentation is to replace non-Ostensive lower-level sentences by Ostensive first-level sentences.

The feature of Wisdom's theory of philosophical analysis is the rôle that he assigns to Ostentation in the unravelling of philosophical problems. In his contribution to the Aristotelian Society Symposium he distinguishes three different kinds of Analysis, each of which finds its expression in the substitution of one sentence for another. He gives the name *Material Analysis* to the substitution, for sentences about wholes, of sentences mentioning the individual parts of the whole, where the parts are of the same order of ultimacy (as previously defined) as the whole. *E.g.*, material analysis of sentences about awe will be in terms of fear and admiration. All material analysis is 'same-level analysis'. There is another kind of same-level analysis, exemplified in Russell's famous translation of "The author of 'Waverley' is Scotch" by "There is an object which wrote Waverley, nothing but which wrote Waverley, and which is Scotch". The substituted sentences are still in terms of authorship and being Scotch: there is no attempt at a new-level analysis in terms (say) of experiences, sense-data, marks on paper, etc. Wisdom calls this *Formal Analysis*. People are stimulated to make material or formal analyses by certain characteristic defects in sentences. Material analysis is needed where one word is made to serve for two or more distinct things or conceptions; formal analysis is needed when a sentence is telescoped so as to offend against the canon "Separate points separately stated". The difference between the two is that it may take a chemist or a psychologist to make a material analysis, but any intelligent plain man can make a formal analysis if he knows how to say what he wants to say so distinctly that not even the most foolish of philosophers can misunderstand him. The third kind of analysis is Ostentation, sharply distinct from the others because it is new-level, while they are same-level. Now Wisdom calls Ostentation "Philosophical Analysis". By the use of this term he implies the closest possible connexion between Philosophy and Ostentation. In the articles on Logical Constructions he actually says: "Philosophy is Analysis. We carry out analysis by means of Ostentation, that is, by the substitution of more ostensive for less ostensive sentences." Appended to the dictum "Philosophy is Analysis" is the footnote "Apart from the deductive study of Does God exist? Are we immortal? Is the Universe a set of spirits?" It is not clear from this whether

Wisdom thinks that part of philosophy is analysis and the rest of it the deductive study of such questions as Is the Universe a set of spirits? or whether he thinks that the study of these questions, though harmless and amusing, is not properly speaking a part of philosophy at all. What is clear is that he regards philosophical analysis as the main business of the philosopher, and that he identifies it with Ostentation, which he also speaks of as the "prime philosophic instrument". All that he says about how to perform Ostentation is that the philosopher must "pray for insight", and presumably wait for inspiration. (*Psyche*, 1933.)

III. CRITICAL. In Part II. I have tried to explain what Wisdom means by saying that the philosopher's business is to discover the ultimate structure of facts, that this is done by a process of philosophical analysis that finds its expression in Ostentation, and that Ostentation is thus the main tool and the principal task of the philosopher. I now wish to consider critically the important theory implied by these remarks as to the nature of philosophy and philosophic method.

We will start with Wisdom's three kinds of Analysis. There is no doubt that there are these three kinds of Analysis, that they have often been confused, and that it is highly important to distinguish them. But we shall get into a muddle unless we draw another logically prior distinction that Wisdom appears to have overlooked. It is the distinction between a *branch*, a *type*, and a *method* of analysis. Analysis is an activity in which not only the philosopher but also the scientist, the politician, the historian, the economist and other specialists can engage. There are as many *branches* of Analysis as there are branches of enquiry. Philosophical analysis, scientific analysis, musical analysis are all examples of different *branches* of Analysis. Formal, material and new-level analysis are what I call *types* of Analysis. They are not in themselves *methods*; you have to use methods of Analysis to do them. Praying for insight is a *method* of Analysis; reasoning from self-evident premisses is another. There are plenty of parallels to make clear the difference in meaning between the three terms: *e.g.*, *cheating* is an activity in which the card player, the examinee, the tradesman and others can engage. Cheating at cards is a *branch* of cheating; substituting a prepared pack at the last moment is a *type* of cheating; saying to one's opponent "Look at that fellow behind you" and changing the packs while he has his back to you is a *method* of carrying out this type of cheating. In using the terms "branch" "method" "type" in this unusually exact sense, in which they are strictly

non-interchangeable, I believe I am drawing a necessary and important distinction which is obscured by the ordinary loose usage of common speech.

Formal, material and new-level analysis, then, are three *types* of Analysis. It is new-level analysis that I wish to consider here, since Wisdom assigns to it a unique position in Philosophy. When he says "Philosophy is Analysis" he means that Philosophy is new-level analysis, and his alternative name for new-level analysis is "Philosophical Analysis". It also appears from his articles that he regards this type of analysis as the main method of doing Philosophy. (*Logical Constructions*, Part V., Section VII.) In my opinion both these views, apart from their mutual inconsistency, are completely mistaken. New-level analysis is a type of analysis: it is not a method, much less a branch. But Wisdom claims that it is both a method and a branch! Let us take the latter suggestion first. Can Wisdom be confusing the meaning of "Philosophical Analysis"—a term chosen by him to name a particular *type* of Analysis (*cf.* formal and material analysis), with the meaning of "philosophical analysis"—a term in common use for a particular *branch* of Analysis (*cf.* historical and scientific analysis)? Surely Wisdom cannot have made this gross confusion. Yet even if his assertion is merely that to philosophise is to do new-level analysis, and to do new-level analysis is to philosophise, it is still unacceptable on the barest examination.

Let us use "Philosophical Analysis" with capitals to denote the particular type of Analysis that Wisdom is talking about, which he calls Philosophical to distinguish it from other types of Analysis such as Formal and Material; and let us use "philosophical analysis" to denote the particular branch of Analysis that is commonly called philosophical to distinguish it from other branches of Analysis such as historical or scientific analysis. Then Wisdom is either tacitly assuming or deliberately suggesting that all philosophical analysis is Philosophical Analysis, and I think he would add that all Philosophical Analysis is philosophical analysis. From which it would follow that all philosophical analysis is new-level, and all new-level analysis philosophical. This is false, for (1) Not all new-level analysis is philosophical. *E.g.*, "Germany invaded Belgium" (as a statement of historic fact) cannot be replaced by a more ostensive sentence mentioning individual Germans and Belgians until the history of the period has been studied, and it has been established what exactly did happen. The new-level analysis of this sentence is therefore a piece of historical analysis, not of philosophical analysis: and

(2) Not all philosophical analysis is new-level. *E.g.*, " 'A caused B' = 'A-like events are always followed by B-like events' ", whether correct or not is a piece of philosophical analysis. But there is no change of level; the definiens is no more ostensive than the definiendum. I conclude that Wisdom's identification of Philosophical Analysis with philosophical analysis is mistaken, in whatever sense it is understood. It is therefore better to drop the question-begging term "Philosophical Analysis" and stick to the neutral term "new-level analysis".

The suggestion that new-level analysis is a philosophic *method* derives a certain plausibility from the loose way in which the term "method" is used in common speech. Indeed, if Wisdom is using the term in this sense his assertion is obviously undeniable. But there is a very important sense of "method"—the strict sense in which I use "method" as opposed to "branch" and "type"—in which new-level analysis is *not* a philosophic method, but in which some other things *are* philosophic methods: and it is this latter truth that is obscured by using "method" in its ordinary loose sense.

I will now set down my own views about the nature of philosophical analysis, in the course of which this point about method will be elaborated.

To ask What is philosophical analysis? is to ask three questions: (1) What is the scope of philosophical analysis—*i.e.*, which *branch* of Analysis is it? (2) What *types* of Analysis are used in it? (3) What *methods* are employed by philosophers? I shall take these questions in turn.

(1) Philosophical analysis is the branch of Analysis that deals with philosophical questions. We need not here try to define the term "philosophical question", as I take it we all know well enough what philosophy is to be able to distinguish questions that are philosophical from those that are not.

(2) We cannot answer this question so easily. But I think we can quickly destroy Wisdom's view of the matter, which may help us to see how the land lies. The type of Analysis that is used by respectable philosophers is, according to Wisdom, that which consists in taking sentences that express Facts without Sketching them, and substituting for them sentences that express *and* Sketch. From the way 'Fact' and 'Sketch' are used it follows that philosophical analysis must be new-level; and this we have seen is a mistake. But I now wish to raise a more fundamental objection. The material that philosophers work on when they are doing analysis, says Wisdom, is the class of sentences that express Facts. Is it not plain that almost the exact

opposite is true? Whether a sentence expresses a Fact or not is totally irrelevant to whether or not it is the job of the philosopher to prepare a revised version of it. The philosopher is not interested in whether such sentences as "The stone broke the window", "John remembers the Armistice", "I see a penny" express Facts or not: what interests him is the *meaning* of these sentences, or to put it another way, the propositions they express. It is for this reason that he frequently examines them in the form "A caused B", "C remembers D", etc. Indeed, the cardinal difference between the philosopher and the student of the special sciences is that the former is chiefly interested in propositions, while the latter is chiefly interested in Facts. If the philosopher were interested in the sentence "England invaded France" he would be interested in its meaning—in the Proposition *England invaded France*. But the historian is not interested in its meaning, which he knows well enough for his purposes; he only starts to take an interest in it when it expresses a Fact.

I will try to state this more accurately. There is plainly a sense of "meaning" in which any sentence that makes sense has a meaning, whether it expresses a Fact or not. Let us express this by saying that every sentence that makes sense has a *formal meaning*. There is another sense of "meaning" that applies only to sentences that express Facts. In this sense of the term, to give the meaning of such a sentence is to give an accurate account of the Fact it expresses. We can say that every sentence that expresses a Fact has a *material* as well as a formal meaning. If we know the formal meaning of a sentence we know part at least of what its material meaning would be if it had one: *i.e.*, we can deduce from the formal meaning of a sentence that any Fact which it could express would have such-and-such characteristics. It is the formal meaning of sentences that interests the philosopher; in this he differs from students of the special sciences, who take formal meanings for granted, and are concerned only with material meanings.

This will become clearer if we consider more carefully the nature of formal meaning and its importance to the philosopher. It is necessary first to draw a distinction between *immediate* and *ultimate* formal meaning. The immediate formal meaning of a sentence is what a man knows directly he understands the sentence: it follows from the meaning of the elements of the sentence and the way they are arranged. The immediate formal meaning of "I see a penny" is immediately obvious to anyone who understands English (with the possible exception of those philosophers who try to teach the plain man his own language). On the other

hand, the ultimate formal meaning of a sentence is not always obvious, and can often be reached only by careful analysis. It is here that the philosopher steps in. It is his business to work out the ultimate formal meanings of sentences that express philosophical propositions. I will try to define more precisely the term "ultimate formal meaning". The ultimate formal meaning of a sentence, unlike its immediate formal meaning, cannot be deduced from its structure alone: on the other hand, it is independent of any Fact the sentence may express, since it exists whether the sentence expresses a Fact or not. It follows that the ultimate formal meaning must be deducible from the immediate formal meaning together with some other Fact or Facts about the way the Universe works. And this Fact, or these Facts, may be supplied by any of the special sciences, or by Philosophy itself. For example, take the sentence "A caused B". This expresses a philosophical proposition, in so far as questions about causality are philosophical questions, and the philosopher is therefore interested in its ultimate formal meaning. Its immediate formal meaning follows from its structure, and is known as well to the plain man as to the philosopher. But if the philosopher knows, or thinks he can prove, that the relation called "causality" is invariable sequence, then from this Fact together with the immediate formal meaning of "A causes B" he will be able to deduce an ultimate formal meaning of this sentence, of the form "A is, has been, and always will be followed by B", which is not known to the plain man, and which he might very likely refuse to accept. This need not worry the philosopher, for though the plain man is always right about immediate formal meanings, he cannot claim the same infallible insight into ultimate formal meanings. The ultimate formal meaning of a sentence may either appear as a new-level translation of the sentence, *i.e.*, a piece of *new-level* analysis, or as a piece of *material* analysis, as in the case of causation instanced above. (*Formal* analysis is not philosophy; it can be done by any intelligent plain man since it just consists in finding the clearest possible way of expressing the immediate formal meaning of any sentence.) It is, I think, obvious that there are degrees of ultimacy in formal meaning. Also that the immediate formal meaning of a sentence expressing the ultimate formal meaning of some other sentence must be the same as its own ultimate formal meaning.

We are now in a position to answer the second of our three questions about philosophical analysis, namely What types of Analysis are used in it? As might have been expected, this question has proved to be bound up with another question that

we did not ask at all, namely What is the purpose of philosophical analysis? The purpose of philosophical analysis is the discovery of the ultimate formal meanings of the class of sentences that express philosophical propositions, the term being used in such a sense that many quite ordinary sentences like "I see Jones" express philosophical propositions, provided they express propositions which, for whatever reason, excite the professional interest of the philosopher. The type of Analysis that philosophers must use, if they start with this purpose in view, is that which consists in replacing sentences that express philosophical propositions, but whose ultimate formal meanings are not deducible from their structure, by sentences that express the ultimate formal meanings of these sentences. This analysis will sometimes be new-level, and sometimes material.

(3) Now, lastly, we come to philosophical method. What methods are at the philosopher's disposal to assist him in the discovery of ultimate formal meanings? Remember what the philosopher has got to do; he has to discover Facts about the structure of reality which, together with the immediate formal meanings of the sentences that interest him, will enable him to deduce the ultimate formal meanings of these sentences. How is he to discover these Facts when the special sciences cannot supply them?

No doubt different philosophers work differently, but I think one may safely say without giving away any trade secrets that the foundation of philosophic method is deduction from premisses that are self-evident or are known to be true. This is the "deductive metaphysics" that Wisdom relegates to a footnote. He would not have made this mistake if he had distinguished clearly between a type and a method of Analysis. Deductive metaphysics is a method; it is as old as Philosophy itself, but it is never likely to become old-fashioned, since Philosophy would be impossible without it. Its validity depends, of course, on the possibility of the intuitive appreciation of the self-evident; and to this method, for those philosophers whose sense of the self-evident is wanting, or is liable to run away with them, prayers for insight form, no doubt, a valuable adjunct. Others may find black coffee or tobacco more effective. To each his taste.

I do not propose to say any more about philosophic method, since it does not seem to me that Wisdom's theories throw any new light on the subject. I will add one warning. In putting forward the view that Philosophy is concerned with the ultimate formal meanings of certain sentences, whether or not they record Facts, I do not wish to suggest that Philosophy is only concerned

with language; that the philosopher is only a glorified grammarian. It would be as absurd to suggest that the chess player is only concerned with bits of ivory. Sentences are the counters with which the philosopher plays his game: but that game is neither lexicography nor syntax. The philosopher is attempting to discover the truth about the universe—to probe the ultimate nature of Reality. What he actually does is to attempt to give the correct translations of sentences that express propositions of philosophic import. His work must take this form since, like the rest of us, he is constrained to move within the framework of language. Pure thought without language may one day be possible, but philosophers will still have to use language to communicate with one another, and with such of the outside world as have the time or the inclination to listen to them.

I have now tried to answer briefly the question What is philosophical analysis? My answer differs radically from Wisdom's. (1) Wisdom says that philosophy is the attempt to discover the Structure of Facts. I agree with this in so far as it follows from the fact that philosophy is the attempt to discover the ultimate structure of Reality. I disagree with it in so far as it suggests that the philosopher's working material is the class of sentences that express Facts. It is the class of sentences that express philosophical propositions with which the philosopher is to concern himself; whether they express Facts or not is irrelevant. (2) Wisdom says that all philosophical analysis is new-level. I say that some of it is new-level, and some of it material. (3) Wisdom has failed to distinguish between a branch, a type, and a method of Analysis. This failure has resulted in a muddled treatment of the whole subject.

The upshot of this discussion is that I do not think much of Wisdom's theory of philosophical analysis. But I said in my introduction that the development of this theory follows a careful study of the expressive function of language, and I want to emphasise here that in my opinion this study is far the more important feature of Wisdom's work. It is not to be condemned on account of the conclusions drawn from it, and its importance to philosophy cannot be measured by the accuracy of criticisms directed against these conclusions. I have not attempted, in the time at my disposal, to give a critical survey of this part of Wisdom's work, which is in any case worthy of an abler pen; but it must be studied by anyone who pretends to the slightest interest in the development of contemporary philosophy.

III.—ARISTOTLE'S THEORY OF REASON (II.): THE POETIC REASON.

BY FRANK GRANGER.

THE aim of this article is to supplement and complete an article entitled "Aristotle's Theory of Reason", which appeared in *MIND* in 1893. Then an attempt was made to view Aristotle's thought apart from the theistic and supernatural implications read into it by Alexander of Aphrodisias and his successors. Now, after more than forty years, a corresponding attempt is made to understand Aristotle the better on another side by eliminating from his account of the poetic reason the confused and irrelevant implications brought in under the cloak of 'æsthetics'. Baumgarten, to whom the term itself is due, guarded against its misuse by emphasising the obscurity of the concepts included under it. He is, therefore, not to blame for all the futilities which have gathered round the *Poetics* of Aristotle under the guise of æsthetic criticism. But the prophets have spoken in vain. Hegel used the name *Æsthetic*, "because it is nothing but a name";¹ Carlyle ridiculed the *Æsthetic* pose in *Sartor Resartus*; and when an Oxford group of *æsthetes* made an irruption among the London craftsmen of the 'eighties' they were received with an amused and rather contemptuous tolerance. Whistler and William Morris and their respective circles had no use in the workshop for the muddled meanings and the dilettantism which were thrust upon them.

The insight of Hegel guides us back to Aristotle. Hegel retains the name '*Æsthetic*' but limits it to the '*Philosophy of Art*' and excludes the '*beauty of nature*'.² Aristotle places the poetic reason, which corresponds to the *Philosophy of Art*, between the theoretic and the practical reason, and places the study of nature under the theoretic reason thus separating it from the poetic reason.³ We are not dealing with three separate faculties but with three different operations of the same faculty directed

¹ *Æsthetik*, 2nd ed., p. 1.

² *Æsthetik, Einleitung*, tr. Bosanquet, p. 38.

³ *Meta.*, 1025 b, 18ff.

to three different forms of science or art. Whereas theoretic activity is realised in the thinking about its object, and practical reason is realised in the action itself, the poetic arts, we are told, are in general productive of something beyond themselves.¹

This wide application of the term 'poetic' is recorded by Plato in a passage of the *Symposium*. True, he does this in passing, and as an illustration of the way in which sometimes a special case monopolises a name which begins with many other applications. But this appeal to ordinary usage, justifies us in regarding his account of 'poetics' as a commonplace. "Poetry is a matter of wide extent. Every cause of the passage of a thing from not-being to being is 'making'² or poetry. And so the operations which fall under any of the arts, are 'poetry' and the corresponding craftsmen are all 'poets'. Yet they are not called poets but have other names. For one part—that which is concerned with lyric poetry and rhythm—is denominated by the name of the whole. This alone is called 'poetry' and those who possess this part of poetry are called 'poets'."³

Yet even poetry falls short of the divine, and with the arts and handicrafts remains banausic. For "God does not mingle with man" and "all intercourse and converse of gods with men is through prophecy and the art of the priests".⁴ Aristotle, however, parts company with Plato. To Aristotle the human reason was one in kind with the divine reason, exercising for a time a faculty which in God is eternal. And the unity of the human reason involves for Aristotle the association of the theoretic and poetic activities. Further, Plato, speaking of "the passage from not-being to being" implies a process of creation. But the notion of creation is foreign to Aristotle for whom all events consisted in the passage from matter to form or alternatively from the potential to the actual.

It is unfortunate that only one of Aristotle's treatises on the application of the poetic reason has survived, the *Poetics*, and that in a fragmentary form. The opening sentence seems to imply a reference to 'poetry' in itself. But this reference cannot be reconciled with the statement of Plato which we have considered and still less with the Aristotelian method. The passage must clearly be rejected as due to a later editor. For so far is the sequel from carrying it out that Aristotle does not define 'poetry' itself as that which causes the transition from the matter to the form, but limits the term to imitation by means of 'rhythm

¹ *Pol.*, 1254 a, 2.

² Similarly in Old English the poet is the 'maker'.

³ *Op. cit.*, 205 b, c.

⁴ *Op. cit.*, 202.

language and harmony', thus following the usage which Plato traces.

We are able, however, to supplement the *Poetics* by references to the wider meaning of poetry in other Aristotelian writings. The *Magna Moralia* (which, although not by Aristotle, reflect the Aristotelian tradition) put all 'poetry' more or less on a level. Building and carpentry are described as 'poetic';¹ playing the harp as 'practical'. Again, the weaver with his loom, makes something beyond the loom, namely, garments. Hence the loom is a 'poetic' instrument.² Although Aristotle does not share Plato's hostility to poets, he is as little inclined to sentimentalise about them. In the *Poetics* he finds a differentia of tragedy not as 'poetry' but in its effect upon the emotions of fear and pity, thus referring tragedy to psychological medicine.

2. *Differentiation*.—It might indeed be said that the characteristic of the Aristotelian, as distinguished from the Platonic, philosophy was the insistence upon the *differentia* which marks off one species from another species in the same genus. Plato, on the other hand, dwelt by preference on the identity which underlies the assemblage of several species under a single head. He is the mathematician over against Aristotle the biologist.

For Aristotle, the difference is the form or essence as opposed to the genus which is the matter. As we proceed from genus to species, we ultimately reach the final difference which implies all the preceding differences. And the definition sums up the process. It was the diagnosis of differences which expressed itself in Aristotle's remarkable classification of the animal kingdom. It lies outside our immediate scope, however, to follow his zoological studies. For the present we are concerned with the definition of artificial objects, such as, to use modern examples, a musical instrument like the organ, or a machine like a printing-press. The definition of such objects includes the ultimate difference by which, say, one printing-press is distinguished from another. And it is often this ultimate difference that displays craftsmanship at its highest.

His politics, ethics, poetics are the minor part of Aristotle's works, and probably of his interest, and they display the biologist. His impartial classification of the poetic and productive activities will not involve distinctions which are alien to those activities. To his scientific serenity the separable accidents of beauty lay outside the scope of his definitions, and he relegated the emotional appeal to the art of rhetoric. The confused meanings

¹ 1197 a, 5.

² *Pol.*, 1254 a, 2.

included under 'æsthetic' perception found no name in his terminology nor place in his system.

Feeling therefore is an inadequate criterion of poetry. When, therefore, Aristophanes in the *Frogs* depicted Dionysus as the judge in a tragic contest, he set up a standard of criticism which, for all its humour, is capable of a serious application. Dionysus analyses his comparison of Æschylus and Euripides into its intellectual elements, and although he began by wishing for the return of Euripides from the shades, as a critic he gives his vote for Æschylus. Butcher¹ to whom the study of the *Poetics* owes so much, is on the same side when he says "the objective laws of art" (or, as we may now say, 'poetry') "are deduced not from an inquiry into the beautiful, but from an observation of art" ('poetry') "as it is and of the effects which it produces." If only he had followed out this principle, he might have given us the standard edition, by keeping strictly within the technique of Aristotle.

We are thus left with the notion of a differentiation which takes place *within* the circle of our own thought, and *within* the material which experience offers for thought to work upon. We no longer have recourse with Plato to a supramundane sphere of Ideas which are the substantial counterpart to our own shadowy notions, and are the realities behind the appearances upon which our notions more or less vainly work. Let us pass behind Plato and with Aristotle take up the Socratic tradition. We shall find the inquiry illuminating also for our study of the 'poetic' tradition.

3. *Aristotle and Socrates*.—The crisis of Aristotle's life is not only deducible from the change in the direction of his work when he passed from the school of Plato to his own independent thinking. It is also recorded by one of the few phrases in which he breaks through the cloak of impersonality and speaks of himself. Aristotle² lightens the emphasis of his attack on the ideal theory by attributing it rather to the school of Plato than to Plato the individual. But even this could not protect him from the charge of ingratitude. His noble reply confessed the pre-eminence of the claims of truth even over those of friendship, friendship to which he devoted a treatise afterwards incorporated in the *Nicomachean Ethics*.

Socrates, in the pages of Xenophon, his *Memorabilia*, *Æconomicus*, *Symposium*, *Apologia*, must be our starting-point not only in the interpretation of Socrates, but in exploring the origins of

¹ Ed. 1895, p. 152.

² E.N. 1096 a, 13.

Aristotle's philosophy. Xenophon represents more nearly than any of his contemporary writers, the Athenian audience to which Socrates addressed himself.

Aristotle himself distinguishes between the actual Socrates and the same figure as modified by Plato ; in the *Politics* he is careful to quote the Platonic Socrates with reference to the *Republic* and not as a historical personage. In so doing he questions the reliability of Plato's report of his master. And indeed where, in the *Nicomachean Ethics*, he criticises the teaching of Socrates, it is sometimes possible to trace the passage to the *Memorabilia* of Xenophon.

But the elimination of foreign elements from the Socratic tradition is not yet complete. Socrates was not interested in natural science : neither in the attempt to discern principles, nor—what is perhaps more surprising—in the obvious phenomena of botany, zoology and meteorology which by long experience we have learned to regard as commonplace. He viewed them not with the calm observation of the nature lover, but only so far as they had 'a teleological meaning beyond themselves', which imposing phrase is a circumlocution for 'useful to the citizen'. But *useful to the citizen of Athens*. In the succession, Socrates, Plato and Xenophon, Aristotle, Plato alone was the amateur, and therefore the more ready to construct Utopias ; Aristotle the biologist, Xenophon the general, Socrates the sculptor, were craftsmen. It was too great a jump from Plato's axioms to the training of a prince at Stagira or to the training of a citizen at Athens.

Socrates' irony consisted in the fact that he accepted the status of the ordinary citizen and made that the taking-off place for the flights of his intelligence. It is an exaggeration to say that he took no part in politics. He resisted the tyranny of the Thirty at the call of justice, as he withstood the popular impulse which demanded the punishment of the victors at Arginusæ. But he disdained the rhetoric and the subtlety which might have separated him from his fellows, in an attempt at political influence. His language was commonplace and his attitude to life practical. He began by following the calling of his father, that of a sculptor. His group of the Graces was shown at the entrance to the Acropolis four hundred years later.¹ It is reasonable to think that he gained by this technical experience. He could give practical advice to men of business.² And in particular his criticism of Parrhasius the painter and Cliton the sculptor had a relevance

¹ Paus. I., 22, 8.

² Xen. *Mem.*, II., 7.

to the movement of Greek painting and sculpture to a deeper pathos and individualism.¹ But we must eliminate the notion of æsthetics from our picture of Socrates even if thereby we find him "incapable of understanding art in its proper nature".² To the clear intellectual vision of Socrates the 'obscure notions' of æsthetics were meaningless. They were replaced by the critical faculty. The Platonic Socrates, therefore, when he came to the craftsmen, knew that he would find them accomplished in their own trades; he does not blame them for pretending to 'æsthetic' knowledge,³ but for claiming the highest wisdom outside their trades. I find it difficult to regard this report as correct. Socrates laid himself open to the obvious retort that he was a stone-mason himself.

Along with the craftsman came the apprentice. And the test of the craftsman was his ability to train apprentices. We must distinguish between the apprentice and the pupil. The one learns by the acquisition of practical habits under the guidance of a master; the pupil in the current sense learns by rote. Only in a minority of cases are the habits of the apprentice and the verbal acquisitions of the pupil guided by the free workings of the intelligence, in this case the poetic reason. It was to this minority that Socrates appealed. But it is a caricature to represent him as limiting himself to converse with statesmen, moralists and philosophers generally. He was attacked for wasting the time of carpenters, tanners, coppersmiths.⁴ Yet he seems to have had a genuine interest in their business. "Not fields and trees but men in town are my teachers."⁵ In this respect he represented the Athenian genius which gave birth to few naturalists and physicists. Hence his topics were drawn mainly from human qualities and also the artificial accessories of life. It was in this sense that "Socrates used to maintain that those who knew what each real thing was, would also be able to explain it to others".⁶ Aristotle pursues the same thought when he says "the craftsman not only knows what a thing is, but what makes it so", for "generally the sign of knowing anything is the ability to teach it".⁷

Although the things to be defined are artificial rather than natural, the simplicity thus possible is all the more adapted to exercise the reasoning powers of the novice. Further, Socrates' method of 'induction' as illustrated by Xenophon, led the person

¹ Xen. *Mem.*, III., 10.

³ Plato, *Apol.*, 22, d.

⁵ Plato, *Phædrus*, 230 d.

⁷ *Meta.*, I. 981 b, 7.

² Zeller, *Phil. Griech.*, II., i., 72, 4th ed.

⁴ Xen., *Mem.*, I., 2, 37.

⁶ Xen., *Mem.*, IV., 6, 1.

questioned through a consecutive series of trials to some sort of conclusion without employing the dialectical devices of the Platonic Socrates so as to embarrass the victim. Socrates did not, however, completely anticipate the method of assembling *different* instances from which a common formula might be educed. In the passage following the quotation from the *Memorabilia* the nature of piety and again of justice is explained by examining the successive forms which a *single* instance might take.

It was not a far step from Socrates' method of definition and induction to the more scientific method of Aristotle, and an Aristotelian commentator attributed with justice to Socrates the first invention of definition and inductive arguments.¹ The same writer further marked the empirical source of the universals and definitions of Socrates, in which Aristotle agrees with him as against Plato. Aristotle then carried further the Socratic principles (which Socrates exhibited in the terms of everyday life and work) by applying them to the natural phenomena which specially occupied him. But this fact does not invalidate Socrates' claim to originality.

4. *Aristotle's Psychology of Reason*.—Our inquiry into the influence of Socrates upon Aristotle will help us in discussing the psychology of reason, as it is presented in the third book of Aristotle's *De Anima*, of which I will translate the fifth chapter, inserting in italics a few notes.

1. Just as in every nature and for each kind, there is the material element (*which is potentially every nature and every kind*), and this is distinguished from the causal element which is 'poetic' through making everything, like craftsmanship in face of its material, it necessarily follows that these distinctions hold good also of the mind. One kind of reason is passive by becoming (*i.e. being made*) everything, and the other poetic by making everything, an accomplishment which operates like light, for light in its way makes potential colours actual. And this reason is immaterial, impassive, unalloyed and actual in its essence.

2. Now the agent is superior to that on which it acts, and the principle is superior to its material. And actual knowledge (*retains this superiority even although it*) is identical with its object. Potential knowledge is prior in time for the individual, but generally speaking not even in time. And this in spite of the fact that the poetic reason operates at one time and not at

¹ *Meta.*, 1078 b, 28.

another. Only by its separation does it exist in its proper essence which alone is immortal and eternal. Of it, because of its impassive character, we retain no memory, while the passive reason is perishable and does not think anything apart from the 'poetic' reason.

It is remarkable that Aristotle begins with the influence of craftsmanship upon the distinction of form and matter, and in his reference to light brings in also the contribution of natural science to his methodology, namely, the distinction between the potential and the actual. But in his refutation of the Platonic theory of Ideas, Aristotle seems to have been impelled first by the inapplicability of the ideas to craftsmanship: They did not work, and they furnished no help to the 'craftsman'.¹

5. *Socrates' dæmonium*.—A still more important point of contact between Aristotle and Socrates, was the negative character of Socrates' *dæmonium*, the inner voice which restrained but did not urge, and by its negations contributed to the wisdom which his contemporaries recognised in Socrates. This warning voice kept him from the unrestrained flights of the philosophic imagination in which Plato indulged. And this limitation, in Goethe's phrase, is the first, the most important, sign of Socrates' mastership. He could only have lost by going beyond his vocation as a teacher to take part in politics.

In the same way, the observation of the habits of his fellow men in daily life involved abstention from the pursuit of natural science *for him*. His *dæmonium* was, in a literal sense, his vocation to mind his own business first of all, to be a man. It is difficult not to think that Aristotle, in his definition of Virtue, had Socrates specially in mind when he spoke of the *phronimos*.

For the negative attitude is not a bare negation. It is a stage on the way to a positive principle.

At the end of the article of which these pages are a sequel, it was suggested that "it may appear something of an anticlimax to pass from anything so magnificent as a Theory of Creative Reason to Aristotle's experience as a practical teacher".² For Aristotle limits the effect of *teaching* so far that he, in a passage which has been strangely overlooked, almost rejects the term altogether.³ Just as Socrates described himself as only assisting at the birth of a mind or an idea, so Aristotle regarded the teacher as merely the occasional cause of the passage into actuality of what is potentially present. He is not the creator, but the guide of the faculties which, under his teaching, his disciples reveal,

¹ *Meta.*, 1092 b, 26.

² P. 318.

³ *De Anima*, 417 b, 11.

that is to say, he is rather the master of an *apprentice* than the teacher of a pupil.

Aristotle's method of exposition which is that of a critic begins by enumerating the opinions which are to be rejected before he affirms his own, "adopting what is rightly said and on guard against what is not".¹ The negative attitude which represented Socrates' highest degree of intuition is thus the first stage of Aristotle's critical method.

Plato exaggerated this attitude of Socrates and often turned him into a mere partisan. Nor did his own practice always rise to the objectivity demanded by pure dialectic. Hence his contribution to the history of philosophy, compared with that of Aristotle, is limited. For only when opposing opinions are fairly met, is it possible to place a given hypothesis in a proper light. Hence the teacher must be impartial in order to judge and discriminate. That is as far as the Socratic method takes us. But there is something beyond. Mind not only learns (in Greek 'to learn' is a passive and is equivalent to 'is taught'); but mind invents, discovers what is new.² To crown all, it becomes able to find an object of thought in itself.³ And this is the greatest discovery of all, the *γνώθι σεαυτόν*, 'find yourself'.

6. *Invention as a Characteristic of the Poetic Reason.*—We may now trace some of the stages which led up to this climax. Aristotle was an inventor. By reflecting upon mental processes he became the first to enunciate and to explain distinctly the Principles of Contradiction and Excluded Middle.⁴ He was the first to formulate the theory of the Syllogism.⁵

In that same context, he points out how invention in the arts is the most important, the most difficult, the most obscure stage of all, and that the inventor as a rule advances but a little way, leaving to after times the task of completing what he has begun. We may expect therefore that Aristotle will lay stress upon the first invention of technical expedients. In the *Poetics* for example, we find references to the invention of plots, of names and incidents, of tokens by which unrecognised characters are revealed. "But who brought in masks or prologues, or increased the number of actors is unknown."⁶ (It appears from this that Sophocles followed an already existent custom in employing a third actor.)

Invention which we have traced in its application first to logic and then to the details of poetic and especially dramatic

¹ *De Anima*, 403 b, 23.

² *Ibid.*

³ *Soph. El.*, c. xxxiv.

⁴ 429 b, 8.

⁵ Grote, *Aristotle*, 2nd ed., 140.

⁶ 1449 b, 4, i.e. in comedy.

form, takes a wider scope. The Platonic theory of ideas canonised the imitation of those ideas as the typical procedure of the craftsman, and ignored invention. To take a modern instance, Ben Jonson, in his *Discoveries*, did not recognise the element of invention as distinguished from imitation, and while he correctly quotes Aristotle as deriving 'poetry' from 'making', goes on to give 'feigning' instead of invention as an alternative function of poetry.¹ Keats returned to the Aristotelian tradition when he affirmed in a letter to his brother George in 1817 that invention is 'the Polar Star of Poetry', although "this same invention seems indeed of late years to have been forgotten as a Poetical excellence." It is omitted from the indices of all the editions of the *Poetics* which I have consulted. To the best of my knowledge it does not occur in Keble's *Lectures on Poetry*. Prof. Housman's psychology has no room, either, for the poetic reason when he says that "the intellect is not the fount of poetry, that it may actually hinder its production".² On reading this, I felt that Cambridge was still faithful to the Platonic tradition. The Platonic Socrates had already said: "I knew this in a word about the poets that they did not make the poetry they made, by any exercise of the intelligence."³

And yet, Aristotle conceives himself to be following and emending Plato, when he says, in the teeth of the ideal theory, that Plato is right in describing "the soul as the place of forms,"⁴ except that it is not the whole soul but the rational soul, and the forms are not actual but potential".⁵ Whatever there is of poetry, in the narrower sense we are considering at the moment, or in the wider sense, must therefore receive its forms from this source. Invention therefore consists in this: that the individual mind imposes some as yet unrealised form on the material ready to receive it.

To the comprehensive view of Aristotle the several arts presented themselves *sub forma humanitatis*, and were to be considered not so much for the sake of their objects as for their relation to man himself. The human reason displayed in its particular expressions the universals which should be found, if anywhere, in the history of the generalising process. The process of induction itself consists in the tentative application of a formula or pattern to a series of instances until the reason recognises itself in them.

In politics, for example, the inductive method of Aristotle led

¹ *Op. cit.*, 'What is a poet?'

² *The Name and Nature of Poetry.*

³ Plato, *Apol.*, 22, b.

⁴ But cf. *Parmenides*, 132 b-134 b.

⁵ *De Anima*, 429 a, 27.

to his discovery of the development of constitutions, the order in which monarchy, oligarchy, democracy, dictatorship follow one another. We have seen within a short period the sequence of monarchy, democracy, dictatorship in European countries. The attempt of socialism to establish a fixed form of state is illumined by the observations of Aristotle drawn from a distant age but based upon the permanent characteristics of human nature. The extent to which his forecasts have been realised, is a confirmation of the general truth of his speculations.

History is perhaps the most illumined sphere in which we can trace first the contemplation of the material, and then the invention of the method by which the material is sifted by the negative method already described, and then built up into an intelligible form.

The *Constitution of Athens* is a fortunately preserved sample of the mind of Aristotle working upon the material on which he based his political speculations. Like Plato he was haunted by the dream of a small class of highly educated persons who alone enjoy the full benefits of citizenship from which craftsmen are excluded. This conception, drawn more or less remotely from Plato, is more valid than is claimed even by its modern exponents. The control by bureaucracy in the trades generally, in teaching, in religion, as expounded in Plato's *Republic* and Aristotle's *Politics*, is realised in several European states and proposed for our own country. The relation of this control to invention and the poetic reason, involves no inner contradiction in the case of Plato for whom the Ideas reign aloof from human kind.

For Aristotle the individual soul, indeed, is the place of forms. Yet by consigning the vast majority to serfdom, he limits the flow of invention. Hence, even Aristotelianism could not be permanent. His political formulæ could not be fitted to the great states and cities of the successors of Alexander. It was left to the Stoics to elaborate the conception of the world-state and of the world-citizen, and so to give full scope to the Aristotelian theory of the poetic reason.

7. *The Discontinuous Operation of the Poetic Reason.*—A peculiarity of this theory which has sometimes been overlooked, connects it with the theory of inspiration. Aristotle held, according to the most probable reading, that "the poetic reason operates at one time and not at another".¹ Aristotle states an obvious psychological fact. In the greatest masters invention sometimes flags. Homer nods as Longinus points out.² The

¹ *De Anima*, 430 a, 22 (Torstrik).

² *On the Sublime*, c. xxxiii.

partial character of inspiration—to use an ordinary phrase—or to speak more precisely with Aristotle, the intermittent operation of the poetic reason, is shown on a larger stage when we view the course of scientific discovery and mechanical invention in the ancient world. Vitruvius combined a historical survey with his technical expositions. From his ninth and tenth books *On Architecture* we might infer that Ionia, Macedonia, Southern Italy, Sicily were more susceptible to scientific inspiration than Athens. The Athenian mind, so richly endowed in other directions, could only produce Plato himself and Meton the astronomer for the first rank in the history of science.¹

The authority for the reading: “the poetic reason operates at one time and not at another”, instead of “the poetic reason does not operate, etc.” (i.e., “always operates”), is Theophrastus himself. He seems to have found the unnegated reading in his text of Aristotle. For in his commentary upon this passage, he asks “why does not the poetic reason operate continuously?”² Torstrik rightly connects this question with the previous passage of Aristotle, “we must go on to inquire why we do not think always,” (430 a, 5).

By thus dispensing with the notion of a Creative Reason coming in from without, Aristotle's psychology, including that of the reason, remains within human nature. Man is dignified by his own originality. In Aristotle's system the human reason is united to the divine reason not by any relation of dependence, but by likeness of form. God and man exist side by side in the permanent order of the world. Consequently the human judgment at its best can have no authority above it. Some such claim as this is implied in the pursuit of logic and metaphysics. Further, by keeping the poetic reason within the limits we have indicated, Aristotle claims also for the arts as practised by man something of the excellence of Nature at her best. “All craftsmanship, and training in particular, are designed to fill up what is lacking in Nature.”³ It is unnecessary, therefore, to refer the splendours of epic and tragic poetry to inspiration, as though the human reason is inadequate to these tasks.

In a world that is rent by passion, the rationalism of Aristotle can speak but to few ears, and it finds but few voices. The technical manual by Longinus on *The Sublime*—the attribution which commended itself to Ruhnken is defended in my translation—is in the traditions of the *Poetics*; the matter-of-fact reference to the introduction of metaphor, quoted from Aristotle and

¹ Vitruvius, ed. Granger, vol. ii., p. xi.

² Themistius, *Paraphrases*, ed. Spengel, ii., 189.

³ Pol., 1337 a, 1.

Theophrastus.¹ is curiously in tone with the passage in the *Poetics*² which deals with style from the standpoint of grammar. Nothing has more interfered with the proper understanding of Aristotle than the refusal to follow him in the exploration of minute detail and in the collection of particular instances not always beautiful and dignified.³ The poetic impulse in the special sense of tragic drama appealed to Aristotle as a physician concerned with mental ailments, and the minutiae we have just noted, would not be too small for the eye of medical diagnosis.

All the more necessary is it that intellectual vision should be purged of blurring elements. The student of Aristotle has to deal with a man, indeed, to whom 'logic' and 'metaphysics' were unknown terms for what he called 'analytics' and 'first philosophy'. Still more to the point is Aristotle's refusal to recognise a 'creative' reason, and his ignorance—shared by him with the rest of his world—of any possible meaning to correspond to the modern phrases, 'fine arts' and 'aesthetics'.

"Criticism," says Longinus, "is the last offspring of long experience." And there is reason to think that the *Poetics* came late in Aristotle's career. Just as the *Politics* had for their background the description of 158 constitutions, so the *Poetics* followed upon his collection of *Didascaliae* (the records of the results of the dramatic competitions at Athens). Whereas Plato had evolved theories of politics and poetry from his inner consciousness, Aristotle based the treatises which have come down to us on the widest possible inductions. The material of such inductions was indeed partly assembled by Aristotle himself. The greater part, however, was due to students of his school working under his direction, and after his death under the direction of Theophrastus.

Theophrastus carried on the work of research with still greater emphasis on the importance of particular observation. We need not wonder that after ages attributed to the first masters of the Peripatetic school not only work carried on under their direction, but much later productions coming from the same organisation and ultimately, on the ground of analogies often remote, treatises of a spurious character. All this intellectual activity is represented for us by little more than disconnected fragments, and by epitomes scarcely more themselves than random collections. Only on the side of mathematics is Greek antiquity fairly represented. From this part of tradition we may imagine the richness of what we have lost, and picture the Aristotelian and Theophrastean achievement in other fields.

¹ c. xxxii. 3.² c. xx.³ Jaeger, *Aristotle*, 336.

For we must extend our conception of poetics to include the discovery of truth as well as the invention of new artifices in whatever field. This extension is confirmed by the titles of many treatises known only by their names, but interpreted by analogy with the treatises that we know. If we only knew the Aristotelian corpus as it has been preserved by Andronicus, the theoretic reason would occupy a predominant place in the plan of Aristotle. But when we invoke the records of zoological observations, the solutions of mathematical and mechanical problems, the discoveries especially in the realm of logic and its ally rhetoric, we can place a large proportion of the treatises enumerated in the catalogues, under the head of the poetic reason. The authoritative note which characterises the Aristotelian tradition, may be explained if we suppose that the Aristotelian corpus was a kind of encyclopedia, modified by corrections and accretions during years of delivery and exposition in the school. On this supposition, while, generally speaking, we hear the voice of Aristotle in the works we ascribe to him, it is only in special books like the *Constitution of Athens*, that we are in complete contact with the master, and can discern the originality of his utterance.

IV.—DISCUSSIONS.

ANALYSIS IN GREEK GEOMETRY.

THE historians of Greek mathematics are at one about the method that the Greek geometers called analysis.¹ Prof. Cornford, however, has recently rejected their account and offered a new one.² In this note I shall first give the usual view of analysis, then give Prof. Cornford's view, and lastly argue that Prof. Cornford is mistaken and the usual view correct.

The accepted version of analysis is as follows. It was a method of discovery, a method of discovering either the proofs of geometrical propositions or the solutions of geometrical problems. It was followed by a synthesis, and the latter constituted in the first place a check on the analysis, to make sure that there had been no error; but, secondly, provided that there had been no error, it constituted the actual proof or solution for the sake of which the analysis was undertaken. I give here a simple schematic illustration of analysis and synthesis applied to the proof of a proposition.

Suppose I want to prove, if possible, the proposition (1). Then, if I am to work by the method of analysis, I proceed as follows. First I assume that (1) is true; assume, that is, what I really want to prove. Then I consider what follows from (1). Say I find that (1) implies (2). Then I consider what follows from (2). Say I find that (2) implies (3). I go on in this way until I reach a proposition that I already know to be true. Say (5) is such a proposition. It does not matter *how* (5) is known, provided only that it is known and known independently of (1). It may be an axiom, or a theorem previously demonstrated, or an element of the construction. When such a proposition is reached the analysis is over and the synthesis may begin. The synthesis consists in going through the same steps in the reverse order: 'Since (5) is known to be true, therefore (4) is true and therefore (3) is true and therefore (2) is true and therefore (1) is true, which was to be proved'.

¹ I have verified this statement for Hankel (*Zur Geschichte der Mathematik in Alterthum und Mittelalter* (1874), 137-149), Cantor (*Geschichte der Mathematik*, 2nd ed., i., 207 ff.) and Heath (*The Thirteen Books of Euclid's Elements*, i., 137 ff.). None of these three authors mentions any dissenting view about what the method was. The following account of analysis is based on their statements.

² In the course of an article on 'Mathematics and Dialectic in the Republic, VI.-VII.', *MIND*, N.S., xli., 43-47.

For this method to work, the implications must be reciprocal. Not merely must (1) imply (2), but (2) must imply (1). The chain 1-2-3-4-5 must give an unbroken series of necessitations whichever way you proceed along it. In other words, the propositions concerned must be convertible. Convertibility is easy to obtain in mathematics, which consists largely of propositions asserting symmetrical relations (such as equations) and therefore convertible. But if in your analysis you should use any inconvertible propositions, you will discover that you have done so when you try to make your synthesis. You may discover, for example, that (2) does not entail (1) although (1) entails (2). This is the way in which the synthesis tests the analysis. If, on the other hand, the chain 5-4-3-2-1, when taken in this order, makes a necessary inference, your synthesis constitutes the proof that you were seeking; for (5) is independently known to be true, and (5) mediately entails (1), and (1) is what was to be proved.¹

If proposition (5), the last proposition in the analysis, were independently known to be false instead of being independently known to be true, the analysis would have shown us that (1) was false (since true premisses cannot validly lead to a false conclusion), and therefore that our attempt to find a proof of (1) could not succeed, and it would have shown us this without the aid of any synthesis. Thus we may widen our account of analysis and say that it is a method for either discovering the proof of a given proposition or discovering that that proposition cannot be proved because it is false.

The *reductio ad absurdum* is a special case of the method of analysis. If (5) is false, (1) is false; and if (1) is false, the contradictory of (1) is true. We can therefore prove the contradictory of (1) by assuming (1) and showing that it entails (5), which is an absurdity and as such independently known to be false.

The application of the method of analysis to the solution of problems is similar. You assume the problem solved and infer consequences from that assumption until you reach a consequence that you are able to construct. Then you make that construction and proceed backwards to what is required.

Such is the usual view of analysis. I will quote here Heath's translation of a part of Pappus which is one of the main passages on which it is based.

¹ The point that you may be unable to complete your synthesis, and that this is a sign that the analysis was unsuccessful, is not clearly made in any ancient text known to me. In making this point the historians of mathematics are apparently not repeating what the Greeks actually said, but telling us what is involved in what the Greeks said and did. However, I have noticed two passages that may possibly be references to this point. 'Therefore $D \times AC$ is either greater than $BE^2 \times EA$ or equal to it or less than it. If it is greater, there will be no synthesis, as has been shown in the analysis' (Eutocius, see Archimedes (ed. Heiberg), iii., 160, 10). The other passage is Aristotle, *Soph. El.*, 16, 175a 28: 'In diagrams we can sometimes analyse the figure, but not construct it again'.

Analysis then takes that which is sought as if it were admitted and passes from it through its successive consequences (*διὰ τῶν ἐξῆς ἀκολουθῶν*) to something which is admitted as the result of synthesis: for in analysis we assume that which is sought as if it were (already) done (*γεγονός*), and we inquire what it is from which this results, and again what is the antecedent cause of the latter, and so on, until by so retracing our steps we come upon something already known or belonging to the class of first principles, and such a method we call analysis as being solution backwards (*ἀνάπαλιν λύσιν*).

But in synthesis, reversing the process, we take as already done that which was last arrived at in the analysis and, by arranging in their natural order as consequences what were before antecedents, and successively connecting them one with another, we arrive finally at the construction of what was sought; and this we call synthesis.

Now analysis is of two kinds, the one directed to searching for the truth and called *theoretical*, the other directed to finding what we are told to find and called *problematical*. (1) In the *theoretical* kind we assume what is sought as if it were existent and true, after which we pass through its successive consequences (*τῶν ἐξῆς ἀκολουθῶν*), as if they too were true and established by virtue of our hypothesis, to something admitted: then (a), if that something admitted is true, that which is sought will also be true and the proof will correspond in the reverse order to the analysis, but (b), if we come upon something admittedly false, that which is sought will also be false. (2) In the *problematical* kind we assume that which is propounded as if it were known, after which we pass through its successive consequences (*τῶν ἐξῆς ἀκολουθῶν*), taking them as true, up to something admitted: if then (a) what is admitted is possible and obtainable, that is, what mathematicians call *given*, what was originally proposed will also be possible, and the proof will again correspond in reverse order to the analysis, but if (b) we come upon something admittedly impossible, the problem will also be impossible.¹

And here is the same translator's version of a description of analysis and synthesis which, though not by Euclid, is found in the manuscripts of Euclid's *Elements*, XIII.

Analysis is an assumption of that which is sought as if it were admitted <and the passage> through its consequences (*διὰ τῶν ἀκολουθῶν*) to something admitted (to be) true. Synthesis is an assumption of that which is admitted <and the passage> through its consequences (*διὰ τῶν ἀκολουθῶν*) to the finishing or attainment of what is sought.²

So far as I know, there are no other surviving ancient descriptions of analysis nearly as informative as that of Pappus. All the others either take still more knowledge for granted in the reader, or are unintelligible, or may refer to some non-geometrical kind of analysis.³

¹ Heath, *op. cit.*, i., 138. In Hultsch's edition the text appears on pp 634-636.

² *Op. cit.*, i., 138. For the Greek see Euclid (ed. Heiberg), iv., 364-366.

³ There are eight other passages known to me. (1) The passage in Euclid already quoted. (2) Proclus, *Euclid* (ed. Friedlein), 211, 12 ff. (3) *Op.*

I turn now to the second part of my note, the statement of Prof. Cornford's view of analysis. The following is, I hope, a fair and full statement of his view.

The method of analysis does not begin by asking what 1 implies, as the historians assert. It begins by asking *what would imply 1*. If I find that 2 would imply 1, I ask myself whether I know that 2 is true. If I do know this the analysis is over; but if not, I must go on to a second step. And, as before, this second step will not be asking what 2 implies but what would imply 2. The process will continue until I come to something I do know. Let this be 5. Then I have finished the analysis and can make my synthesis: '5 is true, and 5 implies 4 implies 3 implies 2 implies 1, and 1 is what was to be proved.'

On this account analysis is not a process of deduction. I do not deduce 2 from 1. Only when I proceed in the reverse direction and make a synthesis am I performing an inference. In the analysis the activity of my mind is not demonstration but *intuition*. The analysing geometer *divines* the premiss (2) from which the conclusion (1) follows. Proclus was right when he described analysis in terms reminiscent of the upward path of dialectic in Plato's Divided Line, for that upward path is a series of intuitions.¹ 'It is certain that in his account of the dialectical ascent Plato is describing the upward movement of thought which has been illustrated from geometrical analysis.'²

On this account the implications with which analysis deals would not necessarily be reciprocal. There might be no entailment whatever in the direction from 1 to 5. Analysis would be going the wrong way along a one-way street, and synthesis would be coming back in the right direction.

cit., 255, 8 ff. (4) Ammonius, *Comm. in Aristotelis Anal. Prior.* (ed. Wallies), 5, 5 ff., esp. 5, 26 ff. This gives Geminus' account of geometrical analysis and compares it with five other kinds of analysis, in the course of an attempt to answer the question why Aristotle called his book *Analytics*. (5) Anaritus, *Euclid* (ed. Curtze), 89. This gives a Latin translation of Al-Nairizi's Arabic translation of some observations by Hero which may have been about geometrical analysis, but in the course of the two translations Hero's meaning seems to have become unascertainable. (6) Themistius, *Arist. Anal. Post* (ed. Wallies), 26, 23. Prof. Cornford thinks this refers to geometrical analysis (see his article, p. 45). (7) Aristotle, *EN*, iii., 3, 1112b20 ff. Prof. Cornford thinks this too refers to geometrical analysis (*ibid.*, 44). (8) Aristotle, *An. Post*, i., 23. Solmsen apparently thinks that this gives an account of the same kind of analysis as was used in geometry (*Die Entwicklung der aristotelischen Logik und Rhetorik*, 121-124). He probably refers especially to 84b31-85a1. It seems to be the current view that Aristotle uses the word *analysis* in only one sense, and that the sense in question is the geometrical (cf. Einarson, *AJP*, Jan., 1936). But I am not yet convinced even of the doctrine that Aristotle always uses the word in the same sense. The doctrine seems to be descended from Waitz' statement in his commentary, i., 366.

¹ Proclus, *Euclid* (ed. Friedlein), 211, 20.

² MIND, N.S., xli., 47.

In accordance with this view Prof. Cornford criticizes the usual interpretation of the Pappus-passage as follows :—

I gather from Sir T. Heath's discussion of this passage (*Thirteen Books of Euclid*, i., 138) that modern historians of mathematics—'careful studies' by Hankel, Duhamel and Zeuthen, and others by Offerdinger and Cantor are cited—have made nonsense of much of it by misunderstanding the phrase 'the succession of sequent steps' (τῶν ἐξῆς ἀκολουθῶν) as meaning logical 'consequences', as if it were τὰ συμβαίνοντα. Some may have been misled by Gerhardt (Pappus, vii., viii., Halle, 1871), who renders it 'Folgerungen'. They have then been at great pains to show how the premisses of a demonstration can be the consequences of the conclusion. The whole is clear when we see—what Pappus says—that the same sequence of steps is followed in both processes—*upwards* in Analysis, from the consequence to premisses implied in that consequence, and *downwards* in Synthesis, when the steps are reversed to frame the theorem or demonstrate the construction 'in the natural (logical) order'. You cannot follow the same series of steps first one way, then the opposite way, and arrive at logical *consequences* in both directions. And Pappus never said you could. He added ἐξῆς to indicate that the steps 'follow in *succession*' but are not, as ἀκολουθα alone would suggest, logically 'consequent' in the upward direction. In the definitions of Analysis and Synthesis interpolated in Euclid XIII. . . . the phrase διὰ τῶν ἀκολουθῶν is used in the same way: 'Analysis is a taking of the thing sought as admitted (and the passage) *through the sequent steps* to some admitted truth'. Here again it is translated by Heiberg (Teubner edit., III., 365) '*per consequentias*', and by Heath 'through its consequences'. These definitions may have been copied, with abbreviation, from Pappus' statement.¹

Prof. Cornford accordingly renders διὰ τῶν ἐξῆς ἀκολουθῶν as 'through the sequent steps' each of the three times that it occurs in Pappus. His view is that Pappus means a succession that is merely temporal, not also logical. In the synthesis the succession of steps is logical as well as temporal, but in the analysis it is not; for, whereas the synthesis is deduction, the analysis is intuition.

This completes the second part of my note, which was the presentation of Prof. Cornford's view of analysis. In the third and last part, which is the attempt to show that Prof. Cornford is wrong and the traditional view is right, I shall urge three points: first that he is influenced by a doubtful *a priori* principle, second that he cannot account for a vitally important set of texts, and third that his interpretation of Pappus is mistaken.

First, then, I venture to suggest that Prof. Cornford is under the influence of a doubtful *a priori* principle. He has stated the principle himself: 'You cannot follow the same series of steps first one way, then the opposite way, and arrive at logical *consequences* in both directions' (p. 47, n.). If this principle were true, the method of analysis as described by the historians of mathematics would be a *logical impossibility*; and if the Greek geometers really thought they used

¹ *Op. cit.*, 47, n. 1. The reference to Heiberg's ed. of Euclid should be iv., 365.

such a method, they were grossly mistaken either in their geometry or in their methodology. Prof. Cornford is naturally loath to suppose that the great Greek geometers habitually practised a logical absurdity. And if the historians of mathematics also thought it a logical absurdity they would undoubtedly reconsider their ascription of it to the founders of geometry, and try to reinterpret the texts. I cannot help feeling that Prof. Cornford's whole attempt to reinterpret Pappus is due simply to his *a priori* conviction that the meaning commonly attributed to that passage is absurd.

But is it a logical absurdity? In these days when logic is making such surprising developments and so greatly increasing its powers, we may, I think, legitimately marvel at Prof. Cornford's flat statement that the same series of steps will not give logical consequences in either direction, especially when his own university has been so prominent in these new developments. The following three propositions seem to form a series that will give logical consequences in either direction: (1) $3x = 4y$, (2) $3x + y = 5y$, (3) $3x + 2y = 6y$. And when I gave the conventional account of Greek analysis to a mathematical friend he replied that, while he did not see why they called it 'analysis', he himself practised it every day!

That Prof. Cornford's *a priori* principle is doubtful will become much more evident if we turn to our second argument against him, namely that he fails to explain a vitally important set of texts. The fact is that there are two entirely different kinds of text that may be consulted to discover what the Greek geometers meant by analysis. The first is the text that gives a theoretical description of analysis, the text that belongs more properly to logic or methodology than to geometry. In Prof. Cornford's article, and in this note down to the present sentence, only the first kind of text has been discussed. But the Greeks have not left us merely descriptions of their analysis; they have left us also *examples*! And whereas their surviving descriptions are few and meagre, their surviving examples are copious and clear. There are numerous geometrical propositions attacked by the method of analysis in Archimedes' second book *On the Sphere and Cylinder*. There are many such in Pappus himself. Surely we ought to base ourselves on these actual examples rather than on the descriptions. That, at any rate, is how the historians of mathematics formulated their unanimous view.¹

I will translate here a very simple example. I believe it will show clearly, first, that the writer thought he was doing what Prof. Cornford says is impossible, and, second, that in a certain reasonable sense he really was doing that. The definitions of analysis and synthesis found in the manuscripts of Euclid, which we have already mentioned, are followed by proofs of Euclid, XIII., 1-5, by this method,

¹ Hankel's illustration of analysis is actually taken from Pappus, cf. his *Geschichte der Mathematik*, 143, and Pappus 830, Hultsch. For the analytical proofs of Euclid, XIII., 2-5 see Euclid (ed. Heiberg), IV., 368 ff. For Archimedes see Archimedes (ed. Heiberg), i., 191 ff., or Heath's translation.

and I choose the proof of XIII. 1, which says that 'if a straight line be cut in extreme and mean ratio, the square on the greater segment added to the half of the whole is five times the square on the half'.¹

The Analysis and the Synthesis of Prop. 1 without a figure.

For let the straight line AB be cut in extreme and mean ratio at C, and let the greater segment be AC, and let $AD = \frac{1}{2}AB$.



I say that $CD^2 = 5AD^2$.

For, since

$$(1) \dots CD^2 = 5AD^2$$

and

$$(i) \dots CD^2 = CA^2 + AD^2 + 2CA \times AD \text{ (II., 4).}$$

therefore

$$(2) \dots CA^2 + AD^2 + 2CA \times AD = 5AD^2.$$

Therefore, by subtraction,

$$(3) \dots CA^2 + 2CA \times AD = 4AD^2.$$

But (since $BA = 2AD$)

$$(ii) \dots BA \times AC = 2CA \times AD.$$

And (since AB has been cut in extreme and mean ratio)

$$(iii) \dots AC^2 = AB \times BC.$$

Therefore

$$(4) \dots BA \times AC + AB \times BC = 4AD^2.$$

But

$$(iv) \dots BA \times AC + AB \times BC = AB^2 \text{ (II., 2).}$$

Therefore

$$(5) \dots AB^2 = 4AD^2$$

and this is true, for $AB = 2AD$ (by construction).

Synthesis.

Now since

$$(5) \dots AB^2 = 4AD^2$$

and

$$(iv) \dots BA^2 = BA \times AC + AB \times BC \text{ (II., 2),}$$

therefore

$$(4) \dots BA \times AC + AB \times BC = 4AD^2.$$

But

$$(ii) \dots BA \times AC = 2DA \times AC$$

and

$$(iii) \dots AB \times BC = AC^2.$$

Therefore

$$(3) \dots AC^2 + 2DA \times AC = 4DA^2,$$

and therefore

$$(2) \dots DA^2 + AC^2 + 2DA \times AC = 5DA^2.$$

But

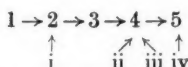
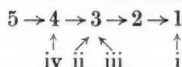
$$(i) \dots DA^2 + AC^2 + 2DA \times AC = CD^2 \text{ (II., 4).}$$

Therefore

$$(1) \dots CD^2 = 5DA^2,$$

which was to be proved.

¹ Euclid (ed. Heiberg), iv., 366, 3 ff. This is not the regular proof of xiii., 1, and is not by Euclid himself, for he does not use analysis. My version is based on Heiberg's Latin translation, *loc. cit.*

Analysis.*Synthesis.*

The accompanying diagrams show the nature of the reasoning. In the analysis the implication goes from 1 by way of 2 and 3 and 4 to 5. In the synthesis it goes from 5 by way of 4 and 3 and 2 to 1, which is precisely the reverse. The propositions identified with Roman numerals are necessary to connect the other propositions together; they are not, however, links in the chain but merely pins that hold the links together. Here then there really seems to be a two-way street of inference.

It might be objected that the example is not really a two-way street of inference precisely because of the presence of the propositions with Roman numerals. The so-called inference from 1 to 2 is really an inference from $1 + \text{i}$ to 2; and therefore it is not the reverse of the so-called inference from 2 to 1, for the latter is really the inference from $2 + \text{i}$ to 1.

To this objection two things may be said. In the first place, we all think in enthymemes; that is, we regard A as following from B when it really follows from $B + C$. Even when we are conscious of the necessity for C, as in the above geometrical example, we often regard A as following from B rather than from $B + C$; and the reason for this is sometimes that C is an old and standing element of our thought, a presupposition under which all our thinking proceeds, while A and B are new elements; they are the actual present process of our thinking. Thus in the above example anything previously proved in Euclid II functions here not as a stage in the present demonstration but as a condition controlling the course of it. The construction functions in the same way.

If our tendency to regard some of our premisses as being the banks of the stream of thought and not any part of the water—if this tendency involves an erroneous view of thinking, then the above example is not really a series of identical steps taken first one way and then the other, and the author was mistaken in supposing that it was, and all the Greek geometers were mistaken in so far as their analyses and syntheses demanded adventitious premisses lying outside the series itself. Along this line of thought, therefore, we answer the objection by more or less admitting it, by admitting, that is, that in a really strict use of language the synthesis does not traverse exactly the same steps as the analysis.

The second answer consists in pointing to one particular step in our example, namely that from 2 to 3. Here there is no adventitious proposition, and therefore apparently the inference in the synthesis from 3 to 2 is the precise reverse. However, it might be maintained that even here there is a suppressed major premiss, the presence of

which will make the synthesis different from the analysis. Whether this must always be true in the last resort is a difficult question of logic to which I do not know the answer ; but I believe I have pursued the matter far enough to show two things : first, that in an ultimate and strict use of language Professor Cornford's *a priori* principle may be true ; and, second, that in ordinary language there is a very natural and reasonable sense in which his principle is false. This conclusion is sufficient to vindicate the historians of mathematics from the charge of making ' nonsense ' of Pappus. It remains to see whether they have ' lamentably misunderstood ' him ;¹ and that brings us to our third and last point, the argument that Prof. Cornford's interpretation of Pappus is mistaken.

We have established the conclusions (1) that you *can*, in ordinary language, ' follow the same series of steps first one way, then the opposite way, and arrive at logical *consequences* in both directions ' ; and (2) that the Greek geometers frequently did so, and called the procedure analysis and synthesis. Hence there is no longer any objection to finding this meaning in Pappus, if it seems to be what he says. Now surely it does seem to be what he says. Τῶν ἐξῆς ἀκολουθῶν is more likely to mean a succession of logical consequences than a succession of sequent steps that are not logical consequences. Surely Prof. Cornford has preferred the latter meaning only because he thinks that the former gives a logical monstrosity.

Prof. Cornford leans heavily on the word ἐξῆς to support his interpretation of ἀκολουθῶν. Pappus ' added ἐξῆς to indicate that the steps " follow in succession " but are not, as ἀκόλουθα alone would suggest, logically " consequent " in the upward direction '. Yet in the Euclid-passage he is obliged to give the same sense to ἀκολουθῶν although there is no ἐξῆς ; and hence he is obliged to suggest that the Euclid-passage was copied from Pappus, ' with abbreviation '. According to the historians, however, the Euclid-passage goes back to Eudoxus or at least to Hero, both of whom lived before Pappus.²

Prof. Cornford has overlooked the fact that on his interpretation Pappus makes a mistake in logic. Pappus says that when, in theoretical analysis, we reach a *ὁμολογούμενον* or something admitted (*sc.* admitted to be true or admitted to be false), then, if it is admittedly false, the conclusion sought will be false also. This does not follow, on Prof. Cornford's interpretation ; for if in the chain 1-2-3-4-5 the implication holds only from 5 to 1, and not also from 1 to 5, then it is possible for 5 to be false and 1 to be nevertheless true, since false premisses can give rise to true conclusions. Pappus makes a corresponding statement about problematical analysis a few lines lower, and therefore, on Prof. Cornford's interpretation, he makes the same mistake again. On the ordinary interpretation of analysis, however, since 1-2-3-4-5 is a chain of necessary implications whichever

¹ *Op. cit.*, 46.

² The theories are summarized in Heath's *Euclid*, I., 137.

way you take it, Pappus is correct in saying that the falsity of 5 involves the falsity of 1.

There are two phrases in the Pappus-passage that at first sight favour Prof. Cornford's interpretation, and I will conclude by examining them. 'In analysis we assume that which is sought . . . and inquire what it is from which this results, and again what is the antecedent cause of the latter, and so on, until by so retracing our steps we come upon something already known.' On the conventional view of analysis we might have expected him to say 'what results from this' instead of 'what it is from which this results'; and if this sentence were our only evidence about analysis Prof. Cornford's account of that method would be preferable. On the other hand, this sentence is not incorrect on the usual view of analysis; it is merely unexpected. Since on the usual view the implication goes both ways Pappus would be correct whichever way he said it went. And his own phrase τῶν ἐξ ἧς ἀκολουθῶν, and above all the examples, show what the truth is. The reason why he expresses himself here in this unexpected way is that he is looking at analysis as existing for the sake of synthesis; this makes him describe the steps of the analysis, not as they appear while you are doing the analysis, but as they appear in the subsequent synthesis.

The other thing which at first sight favours Prof. Cornford's interpretation is the statement that synthesis takes the steps in their 'natural order'. But this can be given a good sense on the accepted view of analysis. The order in which propositions are taken in analysis is 'unnatural', in spite of the fact that it gives a necessary connection, because you start with a proposition that you do not know to be true and treat it as if you did know it to be true. This unnaturalness is very clear, in the example that we have studied, at the beginning of the analysis. The proposition that is set out in one line as to be proved is stated in the very next line as if it were already known.

I urge that these considerations sustain the historians' interpretation of the Pappus-passage against that of Prof. Cornford.

RICHARD ROBINSON.

MR. RYLE AND THE ONTOLOGICAL ARGUMENT.

Mr. Gilbert Ryle in his recent article ¹ has treated a contention of Prof. Collingwood's as a challenge and has met it with a very closely reasoned criticism of the Ontological Argument, which seems to me to miss the essential point of Hegel's defence of the doctrine. In his anxiety to expose its formal defects, Mr. Ryle, not unlike Kant, has failed to see the philosophical truth underlying and inspiring the Ontological Argument and, at the same time, has overlooked the fact that Hegel was aware of the shortcomings of the Argument as well as of its deeper truth. His criticism leaves Hegel's position untouched, and in consequence Prof. Collingwood's contention (namely, that so far as Hegel did and intended to reinstate the doctrine his position still holds good) may stand—even in spite of Mr. Ryle's criticism.

Prof. Collingwood clearly was not much concerned to defend a doctrine which he considered to be in no need of a champion, so it is not with Mr. Ryle's particular criticism of him that I wish to deal. I shall concern myself simply with the position as Hegel left it, and if I do no more than repeat his view, it is because I think little else is necessary to show Mr. Ryle's shortcoming. Mr. Ryle's argument is, in effect, very like Kant's; and Hegel claims to refute Kant's. Yet I cannot see that Mr. Ryle has done anything special to meet Hegel's criticism of a view so like his own. I shall be content, therefore, though I may not succeed in throwing new light on the matter, if I can at least show that the Hegelian view of the Ontological Argument is still intact.

Mr. Ryle's criticism may be summed up as follows: Existence is not and cannot be made a "predicate" and the Ontological Argument depends for its validity on just this illegitimate treatment of existence as if it were a "predicate". His own words are: "But unfortunately [for Mr. Collingwood] this is precisely where I should have thought not only Hume and Kant but almost all recent logicians who have attended to the analysis of existential propositions would dig their heels in and say that the argument is an obvious fallacy *unless* existence is a 'predicate'; and that existence is not a 'predicate'. . . . How can particular matters of fact be deduced from *a priori* or non-empirical premisses?"

What is meant by saying that existence is not a predicate is, I presume, that existence is not part of the character—the "what"

¹ MIND, vol. xliv., no. 174.

(to use a Bradleian term)—of the subject which is asserted to exist. All the possible characters or "predicates" that can be attributed to it constitute its "what", but *that* it is is something beyond and, so to speak, external to its "what". I should be unwilling to agree that this is entirely and finally true, but so much may be admitted that to say, in bare abstraction, that X exists adds nothing to our conception of X. Hence Hegel is led to remark, "if we look at the thought it holds, nothing can be more insignificant than being".¹ But if the denial that existence is a predicate comes to this, our conclusion should be not simply a refutation of the Ontological Argument but rather that the proof of God's bare existence is of no philosophical importance whatsoever, if only because of the extreme emptiness of the apparent predicate. So far as Mr. Ryle's view serves to suggest something of this sort it is not without value.

But the matter may be regarded in a different light which reveals a deeper truth in the Ontological Argument and one which Mr. Ryle's criticism entirely overlooks. He protests that "matters of fact" cannot be deduced from "non-empirical premisses". These he defines as "premisses about the characteristics the combination of which is symbolised by the description"—of something whose existence (in this instance) is to be proved. But he does not say what he means by "matters of fact" nor does he tell us positively what sort of premisses they could be deduced from. Presumably, these would be "empirical" premisses. Now premisses "about the characters" of things are, I take it, such premisses as arise out of classification of things according to their characters (*e.g.*, propositions expressing the relation of species to genus), such as analyse the characters themselves, or state necessary connections between them. These are matters not given directly in sense-perception. But what is presented in sense-perception may, I suppose, be regarded as a "matter of fact" and propositions stating perceived facts will be "empirical" premisses. I shall assume, therefore (I hope without doing violence to his view) that Mr. Ryle is prepared to admit as proper to a proof of existence the sort of premiss which states a fact given in sense-perception.

The question must then be raised: What is the nature of the evidence provided by sense-perception for truths of fact? Let us consider an example. I am assured of the existence of this pen because I can feel it, see it, make marks on the paper with it which other people can see, and so forth. All these facts are the evidences of sense-perception. None of them is simple, but I do not intend to discuss the nature of their complexity. What is important to notice is that the existence of the pen is not adequately proved by any one of them alone but can be proved only by all or a number of them taken together. An isolated perception can prove the existence of nothing, except a momentary state of consciousness. That I now seem to see a pen is not sufficient to justify my making the judgement,

¹ *Encyclopædie*, § 51; Wallace's Translation.

"That is a pen", for my visual sensations may be produced by curious effects of lighting or by anything that simply *looks like* a pen but is not one, or may even be imaginary. Nor would the judgement be justified by any number of different perceptions unless the bearing of each upon the fact to be proved reciprocally gave significance to and received it from all the rest. The fact of the existence of the pen is proved by the mutual corroboration of several perceptions which together provide a body of evidence. In the example taken, I can conclude from what I perceive to the existence of the pen because all my perceptions of it and of matters relative to it reveal to my mind a number of facts (that there is here something which feels smooth and hard, which appears black, which both looks and feels cylindrical, which I can handle and use, etc.). These facts form a system to complete which the fact of the pen's existence is necessary. The evidence is such that if it is accepted the conclusion must follow. The further inquiry why it is necessary or how it is even possible to accept the evidence simply on the strength of sense-perception would lead us into a discussion upon which I do not wish to embark in this article. I shall say only that I believe the answer is to be found in the fact that all judgements of perception are interpretations of what comes to us through sense in the light of the systematic conception of the world which we have built up in the course of our experience. This interpretative character gives value as evidence to what we perceive. Our experience of the world of things in space and time is the experience of an ordered world ; of a world, that is, in which certain laws and principles are found to hold good (even before they come to be formulated abstractly in reflection). If this were not so, no valid reasoning from experienced "facts" to any other "matters of fact" would be at all possible, for all inference must rest on a system and the deduction of "matters of fact" rests on the systematic character of the experienced world. Accordingly, we may, on the basis of this system, reason from a set of facts (such as the body of evidence described above) to a conclusion which the nature of the evidence, on that basis, requires. We should say, in effect (if we made the argument explicit), "Things being what they are, I cannot, in the face of this evidence, deny that X exists".

The establishment of a fact, then, depends first on a body of evidence, and secondly on the ordered system of the experienced world. To prove the existence of a thing we must show on sufficient evidence that the thing is a part of the system of things in space and time. The evidence is sufficient when to deny the conclusion to which it leads would disorganise the system. The necessity of the inference is due to the system, and lies ultimately in the impossibility of rejecting the system in its entirety.¹

Here we have the crux of the matter, but before proceeding certain points already reached must be emphasised :

¹ Cf. Bosanquet, *Implication and Linear Inference*, chap. i.

(1) Mere sense-perception cannot prove the existence of anything other than momentary states of consciousness.

(2) No judgement of perception by itself can prove a matter of fact; nor can any number of such judgements, except by demonstrating a body of evidence from which we can *infer* to the fact in question.

(3) This is true even when the matter to be proved is the existence of something at the time present to the senses.

Accordingly we must conclude that, whether or not Mr. Ryle is justified in demanding "empirical" premisses for deducing "matters of fact", the essential element of the deduction (what has been called "the nerve of the inference"¹) is not the so-called "empirical" character of the premisses but the systematic character of the evidence which they contain.

To resume, however, the crux of the matter is that whatever doubts we may entertain about particular matters of fact, these doubts must be dispelled when they involve a challenge to the ordered character of the whole experienced world. The proof of any existence must stand, if it is to stand at all, upon that order, and there is no other means beyond the world as a whole of proving an existence which the evidences demonstrable within the experienced world disprove, or *vice versa*. When we are faced with the alternative, "this fact or nothing", we must choose "this fact" or commit intellectual suicide. In other words, whatever particular facts we may deny or doubt, what we cannot possibly deny or doubt is the whole world of fact, for upon it any denial must depend for its validity and any doubt for its justification.

Now those who believe that Hegel reinterpreted the Ontological Argument in a manner which put it beyond the reach of such attacks as that of Kant hold that our experience of finite things existing in space and time, though systematic enough for everyday purposes, reveals itself on reflection to be a very incomplete and imperfect system. Yet, they hold, it is one which, we find as we try to understand it, implies a wider and more perfect system. In fact, they go as far as to say, that if the world of everyday experience is to be intelligible ultimately (*i.e.* philosophically) it must be regarded as a part, or an aspect, or an appearance (according to the view taken) of an Absolute Whole of reality transcending the experience of finite things. This Absolute is to be identified with what the authors of the Ontological Argument call the most perfect being, and clearly to deny the existence of this Whole is a far more serious matter than to deny the existence of what is no more than a part of it, namely, the world of things in space and time. That, we saw, could not be denied because the proof or denial of any and every finite existence must stand upon it. How much less, then, is it possible to deny the existence of that on which the intelligible reality of the whole world of finite existence depends.

¹ Cf. Bosanquet, *Implication and Linear Inference*, chap. i.

This is the source of the Ontological Argument's truth and, in whatever form that Argument may be presented, this is what it ought to mean. If "God" means the most complete and perfect being He (or It) must be identified with the absolute whole of Reality; and the existence of that whole our intellect demands as the logical condition of intelligibility of all our experience. The true meaning of the assertion that God's essence implies existence is that to be infinite and all-inclusive (the character or "essence" of God) is to be ultimate; on what is ultimate depends all existence and all proof of existence; therefore, there can be no denial of the existence of God for there is nothing on which such denial could rest.

The Kantian criticism of the Ontological Argument, with which, as I understand him, Mr. Ryle wishes to associate his own, is that a demand of the intellect is no proof of existence, and we may not argue that because the intellect demands a complete system, therefore there is one. But this criticism breaks down in the face of what has gone before. No proof of existence is ever anything but the satisfaction of a demand of the intellect. We have seen that mere sense-perception cannot prove matters of fact. Such proof requires, at least, the comprehension of sense data into a system from which we can infer to the existence in question. It is only by such inference that any existence can be unassailably established (even when the thing in question is present to our senses). Any such inference, to be valid, must satisfy the demands of the intellect, and the conclusion of such inference is no more nor less than what the intellect demands in the face of the evidence produced. If, then, our experience is such that, for it to have any recognisable character at all and be more than mere meaningless chaos, our intellect demands an absolutely whole system of reality, and if the satisfaction of this demand is the *sine qua non* of the validity of all arguments, including proofs of existence of finite things, then the absolutely complete system of reality must be.

On this view it follows, also, that we may not argue, as Mr. Ryle does, that because the proof of existence of a finite thing requires premisses of a certain kind, therefore the proof of God's existence must require premisses of the same kind. For any operation in thought upon any premisses requires and must rest upon the existence of God in the manner explained. Apart from the Absolute no proof or argument would be intelligible (let alone valid).

If, as is probable, Mr. Ryle would deny the truth of the Hegelian view of Reality, the onus is on him to show where it breaks down, but that is not the point at issue here. The point is that his argument that "matters of fact" cannot be deduced from "non-empirical" premisses will not survive examination in this connection. Even if we overlook the fact that Mr. Ryle has done nothing to show whether, or why, the distinction of "empirical" from "non-empirical" is ultimate, and even if we assume that perceptual experience is all the one and not at all the other (questions not hitherto raised),

it yet has to be proved by Mr. Ryle that to make any perceptual experience intelligible it need not be interpreted in the light of the ordered whole of the experienced world and that in being further interpreted it will not eventually come to be viewed as a part or an appearance of an Absolute Reality which transcends it; it yet has to be proved that "empirical" premisses themselves are not, for the purposes of any proof, devoid of significance except as interpreted by reference to a systematic whole of reality. For if they are, we must still maintain that the existence of that whole is guaranteed by its very nature (or "essence") as the ultimate basis of all partial experiences.

Only if we conceive God under finite categories (as so many well-meaning religious people do) will Mr. Ryle's claim have any plausibility. If in attempting to prove God's existence we were attempting to prove the existence of a finite being (whether or not we call Him finite), Mr. Ryle's condition for the validity of the argument might be admitted. In that case, clearly, His essence would not involve existence for He would not be ultimate and we should have to argue from finite matters of fact. But the Absolute Whole is quite a different matter. The conception of the whole is a conception of something the reality of which cannot be doubted just because it is what it is—namely, that whole in which everything has its being. It is this truth that Hegel wished to indicate in his argument against Kant when he objected to the example of the "*hundert Taler*", saying that in God "we have an object of another kind than any *hundert Taler* and unlike any one particular notion, representation or whatever else it may be called". And it is this truth which Mr. Ryle overlooks.

Moreover, it is here that we see what is at once the strength and the weakness of Mr. Ryle's position. The narrow sense of existence, that in which it applies to finite things in space and time, is not applicable to God (unless in a very special manner which we need not discuss here). So far is existence, in this sense, from being implied in God's essence that the contrary is rather the case. To quote Hegel again, "Existence is . . . a term too low for the Absolute Idea, and unworthy of God". That should be clear from what has been said above, for if God is the whole, and existence means no more than a place in a limited and abstracted scheme within the whole (the spatio-temporal scheme), God clearly does not exist. God is not in existence so much as existence is in God.

Now when Mr. Ryle talks of the illegitimacy of deducing existence from "non-empirical" premisses, when he says that it is not a predicate and cannot be proved *a priori*, he is referring to existence in just this narrow sense. And he is quite right, therefore, to deny that such existence can be proved to belong to God. The sort of demonstration that can prove the existence of finite things, namely, reasoning from "particular matters of fact" cannot be applied to God, for God is not a particular nor is His existence a "matter of

fact" (in the sense that the existence of this pen is one). God possesses reality of another order.

But this again is just the strength of the Ontological Argument—that it is not an attempt to prove from "particular matters of fact" that God has existence like finite things, and the weakness of Mr. Ryle's position is his complaint that the Ontological Argument is not such an attempt. Presumably, had the argument proceeded from "empirical" premisses, Mr. Ryle would have made no objection to its method, whether or not he would have had much hope for its success. But the older philosophers were wiser than to attempt such a proof. They knew very well that God was not to be treated in the same way as finite things and it is for that very reason, because God is infinite, that we cannot regard the idea of God (or, as Hegel would say, simply "The Idea") as something merely "subjective" lacking objective reality. Mr. Ryle's complaint, therefore, does not touch the Ontological Argument any more than Kant's did, and it leaves the whole matter in just that position which justified Prof. Collingwood's attitude.

E. E. HARRIS.

THE LOGICAL PARADOXES.

IN vol. xlv., N.S., No. 178, of MIND, M. Ch. Perelman has published a discussion-article, *Les paradoxes de la logique*. The following remarks aim at showing that the solution proposed by M. Perelman for these paradoxes does not remove the doubts the well-known contradictions have raised about the consistency of classical logic.

In discussing M. Perelman's paper I wish to discard at the outset two of the paradoxes mentioned by him. These are the "barber" and the "paradox of relations". The paradox of the barber is a joke of the same kind as many so-called paradoxes by which the ancient sophists puzzled their public and each other. If *e.g.*, the company-barber is defined as that man of the company who is obliged to shave every man not shaving himself and no other, then there can be no man fulfilling exactly this duty, because it is defined contradictorily. The solving of this paradox may perhaps be a useful exercise for young students of logic; but it does not offer any scientific interest at all. The paradox of relations on the other hand, although Whitehead and Russell deal with it in the Introduction of *Principia*, is to my mind nothing but the form of possible paradoxes. This seems to me to follow from the occurrence of the real variable T in the formulation of the contradiction it leads to. This contradiction only proves—so far I fully agree with M. Perelman—that there cannot exist any relations R , S and T with the properties ascribed to them in the "paradox". It can be shown, it is true, that certain constant relations in classical logic have such properties, and thereof genuine paradoxes arise; but these—so I believe—cannot be solved in the manner proposed by M. Perelman.

Before entering on the detailed discussion of M. Perelman's solution of the paradoxes I wish to make a further preliminary remark. Every reader of his article will be much impressed by the bewildering simplicity of his solution. One cannot help, as one reads it, being reminded of Christopher Columbus' famous egg. Now certainly I will not deny *a priori* the possibility of M. Perelman's being a Columbus; nevertheless there is an important difference between him and the discoverer of America. The latter had set himself—so the tradition reports—the problem he solved, and therefore also was in a position to determine if his procedure, which did not occur to any of his listeners, was a genuine solution of his problem. M. Perelman's situation is far more unfavourable. The problem he has—a new Oedipus—boldly undertaken to solve, has been set by

logic itself, and its solution also must be adjudicated before the judgement-seat of logic. But the more difficult the task is, the greater would be the merit of accomplishing what such excellent thinkers—to mention the most illustrious only—as Frege, Russell and Hilbert have tried in vain: namely, to *avoid the logical paradoxes without infringing classical logic*.

The main content of M. Perelman's paper may be briefly expounded as follows: The origin of the paradoxes does not lie, as many people believe, in a contradiction contained in the basis of classical logic. The avoiding of the paradoxes therefore does not call for a reform of that basis. But each paradox has itself contradictory premises, the contradiction being, it is true, introduced in a concealed way. Thus the revealing of the concealed contradiction is sufficient to remove the paradox.

If a contradiction appears at the end of a chain of inferences one at least of two cases must be realised according to a very evident theorem of metalogic: (1) either there is a fallacy in the chain, *i.e.*, there is a link in the chain the transition from which to the following link is contrary to the rules of inference, or (2) the premises are contradictory. Now it is very improbable that the former of these two cases should be realised in the well-known logical paradoxes, because one can hardly suppose that such a fallacy could have escaped so many sagacious logicians as have dealt with the paradoxes during more than forty years. Supposing therefore the inferences of the chain to be intrinsically correct, the contradictory result proves that a contradiction is contained in the premises. But on the other hand there would be no paradox if the contradiction in the premises were obvious; since nobody would find paradoxical a contradictory conclusion following from openly contradictory premises. Thus it follows that the contradiction must be contained in the premises in a concealed way. Now I do not know of any other method of revealing a concealed contradiction than that of deducing by logical inferences a contradictory conclusion from the suspected propositions. That is the well-known method of indirect proof used already by Euclid. Thus we can regard every paradox as a proof of the falsity of at least one of its premises. What now makes the essence of a paradox is the fact that it follows from premises appearing to be evidently true. This character of the paradox is thrown into still more relief when the premises are *analytical, i.e., when they follow from the axioms of logic*. According to the opinion prevalent till now this is just the characteristic property of the so-called *logical paradoxes*. It is for this very reason that these paradoxes have been given the name of "logical". It seems to me that M. Perelman has overlooked this fact. In any case he does not mention it at all in his paper.

That the premises of the logical paradoxes are analytical has been proved by several authors who have dealt with the paradoxes. One can find this proof, for instance, in a paper published by the

author of the present article together with Leonard Nelson in 1908.¹ But since, in the meantime, much progress has been made in logical researches, and I cannot therefore maintain everything I wrote twenty-eight years ago, I will repeat the proof for two of the paradoxes M. Perelman deals with in his paper.

Let us first consider the Epimenides or *the liar*, as the oldest of all the logical paradoxes, used already in ancient Greek textbooks as an instance of a sophism. It has been put in a very elegant form by Lukasiewicz :

Let the letter *q* be an abbreviation for the phrase "the proposition occupying the 12th line of this page". Then let us write

(1) *q is a false proposition.*

By counting the lines we verify :

(2) *q is identical with the proposition (1).*

As definition of the concept "false" we assume, in accordance with M. Perelman, Def. : "*p*" is a false proposition is equivalent to non-*p*. By substituting *q* for the variable *p* in this definition we get :

(3) *q is a false proposition* is equivalent to non-*q*.

The first member of this equivalence (printed in italics) is our proposition (1). Thus we have :

(4) The proposition (1) is equivalent to non-*q*.

But in virtue of (2) *the proposition* (1) can be replaced by *q*. Thus results the contradiction :

(5) *q is equivalent to non-q.*

The operations performed by us in this deduction are all legitimate in classical logic. I.e., according to classical logic they lead from true premises to true conclusions. We have substituted the constant *q* for the variable *p* in the definition of a false proposition, and we have replaced the proposition (1) in (4) by the description *q* (this letter was introduced only for the sake of typographical brevity) whose identity with the proposition (1) can be verified by the reader himself.

In his analysis of *the liar* M. Perelman writes : "Or pour que cette opération de remplacement soit possible, il faut que l'on ait affirmé l'équivalence de non-*p* et de *q* quelle que soit la valeur de *p*, ou (*p*). $\sim p \equiv q$. Or cette proposition est fausse." Here the author seems to have forgotten that he has (11 lines before) introduced the letter *q* as a name for the proposition *la proposition p est fausse*. By substituting this for *q* the equivalence criticised by him becomes

"*la proposition p est fausse* équivaut à non-*p*."

But this is the definition of "false proposition" M. Perelman adopted himself by writing : "Affirmer que la proposition *p* est 'fausse' revient à affirmer non-*p*". If he wants to reject it now, what other definition not leading to contradiction will he propose ?

¹ Grelling und Nelson, *Bemerkungen zu den Paradoxien von Russell und Burali-Forti*. *Abhandlungen der Priesschen Schule*, NF, Bd. 2, S. 300.

In any case on the basis of classical logic there seems to be no other definition of falsehood.

Another objection M. Perelman makes to the equivalence (p). $\sim p \equiv q$ is that it states that a function is equivalent to a constant. This seems to me another indication of his having forgotten the meaning of the letter " q ". Since it was a name for the proposition "*la proposition p est fausse*", and this is obviously a function of the variable p .

The foregoing discussion may be summed up as follows :

The paradox of the liar does not presuppose anything but the rules of classical logic, the definition of falsehood and an easily verifiable empirical proposition.

Let us now examine a second paradox. I choose the one I published for the first time in the above-mentioned paper, namely the paradox of the word *heterological*.

Let x be a word-property and n the name of x in the language L . Then we define *heterological in L* as follows :

(1) Def. : n is heterological in L when and only when n has not the property x in the language L .

The English word *monosyllabic*, for instance, has four syllables, and is therefore not monosyllabic. Hence *monosyllabic* is in the English language heterological. The word *English* on the contrary is itself English and therefore not heterological in the English language.

In virtue of this definition the alleged word-property has received the name "heterological" in English. But in order to avoid confusion between the word and the concept I will designate the word "heterological" by the letter H . Let us now substitute for x in the above definition (1) the word-property *heterological*. Then we must put H for n and get :

(2) H is heterological in the English language when and only when H is not heterological in the English language.

This is an obvious contradiction. According to M. Perelman the contradiction in this case must be contained in the definition (1) and is only revealed by substituting *heterological* for the variable x in that definition. But then I ask : Is it not true that the word "monosyllabic" has a certain property not belonging to the word "English" ? How can this property be defined consistently ? How can a definition be seen to lead to a contradiction by certain substitutions ? Is one to test all possible values of the variable ? And how far must the deduction of consequences be pushed in order to be sure that no contradiction will appear later ? So long as M. Perelman cannot answer these questions we must uphold the contention that his "solution" is nothing but the almost trivial establishment that a contradiction appearing in the conclusion must be contained in the premises.

I will pass over Russell's paradox of the class of all those classes which are not members of themselves, because M. Perelman an-

nounces a slight modification of the concept of class which he says precludes this paradox. Since I do not know this modification I must postpone judgement on his solution of Russell's paradox.

As I believe, the foregoing discussion will have shown that the problem of the logical paradoxes is not quite so easily solved as M. Perelman contends. But I wish not to terminate my article with such a purely negative and unsatisfactory result. I will therefore try to give at least an outline of the theory by which the modern logic so much developed in the last twenty years deals with the problem of the paradoxes.

According to F. P. Ramsey we have to distinguish two groups of logical paradoxes: the first are of a mathematical or logical kind in a narrower sense, whereas the second group are of syntactical character because they assume some reference to language or symbolism. The paradoxes of the former group are characterised by the fact that they can be formulated in symbolic logic, which is not possible for those of the second group. The prototype of the former group is Russell's paradox. It has been formulated symbolically for the first time—as far as I am aware—by Frege in the appendix of the second volume of his *Grundgesetze der Arithmetik*. The "liar" or "Epimenides" is the prototype of the second group. This also comprises Richard's paradox and that of the word "heterological".

Russell has shown how to avoid the paradoxes by his theory of types. But Ramsey made a distinction between the simple theory of types and the ramified one. To avoid the logical paradoxes of the first group the simple theory is sufficient. On the other hand the ramified theory, which has been adopted by Russell and Whitehead in order to preclude the syntactical paradoxes, necessitates the axiom of reducibility because without it great parts of logic and mathematics break down.

But, that axiom being exposed to several objections, many authors have looked for means to dispense with it.

David Hilbert was the first to make the distinction between mathematics and *metamathematics*. By this term he designates the theory dealing with mathematical deductions as its objects. This distinction has proved to be very fruitful. Lukasiewicz, the great Polish logician, has generalised that idea by introducing what he calls *metallogic*, which has to ordinary logic the same relation as metamathematics to mathematics. This whole complex of problems has been systematised by Carnap in his treatise *Logische Syntax der Sprache* and some separate papers. Also Gödel and Tarski have contributed much to the development of these problems. The labour of these logicians has led to important results concerning the syntactical paradoxes. They can now be proved to disappear if one carefully distinguishes between a given language *L* and the language *L'* in which one speaks about *L*. Carnap calls the latter *syntax-language*.

Now the every-day language which is also the language of most sciences is so universal that one can speak about this language in itself (what I am doing at this very moment). In other words : the every-day language contains its own syntax-language. Therefore it is possible to say in the every-day language that certain propositions of this language are true or false. But this, as we have seen above, leads to the paradox of the liar. It is impossible to give a definition of truth and falsehood which at the same time is in accordance with usage and does not lead to that contradiction. In a consistent language propositions of that language itself cannot be said to be true or false. For a formalised language, such as, *e.g.*, symbolic logic, consistency can be secured by exact rules for building up new expressions out of the primitive ones. But in such a language syntactical statements about the language itself cannot be made except in a restricted way ; the concepts *analytic* and *contradictory* in the language *L*, for instance, cannot be defined in *L*, as Carnap has shown. In order to escape from these restrictions one must build up a new language (a so-called *meta-language*) disposing of more means of expressing thoughts than the former. But then there are always problems expressible in every-day language concerning that meta-language but not expressible in it if it is consistent.

Thus we have to take our choice between two incompatible goods. The one is the unrestricted capacity of expressing our thoughts, the other security against ever meeting with a contradiction. The obvious parallel with moral philosophy may be drawn by the reader himself.

KURT GRELLING.

SOME REMARKS ON DR. PERELMAN'S ESSAY ON LOGICAL ANTINOMIES.

It seems to me that Dr. Perelman's attempt in the April number of *MIND* to solve the difficulties introduced by logical antinomies, acute as it is, leads to some remarks, of which I hope a few lines will permit me to give a sufficiently clear exposition. According to Dr. Perelman logical antinomies are commonly considered as originating from hypotheses which are "perfectly valid in classical logic" and from which, notwithstanding their legitimacy, it is possible to infer contradictions; the question then arises: Is it necessary to alter logical laws?

This point of view is rejected by the author; in his opinion, antinomies originate from hypotheses which are not at all valid in classical logic; consequently there can be no necessity whatever to alter logical laws (with exception, as it seems, of a slight modification of the definition of class).

The value of this solution is, of course, wholly dependent on the meaning of the words "hypothesis valid in classical logic"; it is not quite clear to me what Dr. Perelman does exactly mean by this expression. Therefore I shall distinguish two interpretations (the only possible ones, as far as I see). It is obvious, by the way, that Dr. Perelman considers "classical logic" as a deductive system *L*, analogous to the one, *e.g.*, given by Peano in his "Formulaire".

1. A hypothesis *H* may be called "valid in classical logic" when it is *compatible* with the system *L*, *viz.*, when the system *L* does not permit to prove $\sim H$.

2. A hypothesis *H* may be called "valid in classical logic", when it is capable of a formal proof in the system *L*; in this case the name is not quite suitably chosen.

If the first meaning is accepted, the quoted problem is easily solved: the supposition of the legitimacy of *H* simply turns out to have been a mistake; therefore (in accordance with Dr. Perelman's solution) the hypothesis *H* must be rejected and the system *L* itself remains untouched.

The second interpretation, however, gives rise to a problem of the utmost importance; consequently it will be necessary to consider this case more in detail. It consists in the supposition of a contradiction inferred from a proposition *H* which is demonstrable in *L*, and thus leads to the self-contradiction of the system *L*. As a matter of fact only this interpretation is relevant to the antinomies established by Russell, Grelling, etc. In this case it is not admissible

to eliminate the proposition H in the manner suggested by Dr. Perelman. Indeed, if the proposition H is demonstrable in L, the contradiction cannot be caused (as Dr. Perelman says) by the *illegitimate* assertion of a formal equivalence, as the assertion of a demonstrable proposition is of course always legitimate.

We may conclude that, if the first interpretation is accepted, Dr. Perelman's method gives a valid means of excluding non-valid hypotheses; in this case, however, no fundamental problem is concerned. In the second case the method suggested by the author does not, as we saw, solve the problem.

It will be clear from what has been said that the result of Dr. Perelman (important though it is in characterising an extensive class of hypotheses which are non-compatible with classical logic) does not render superfluous solutions like those presented by Russell or Behmann; nor does it invalidate Brouwer's profound criticism of classical logic.

E. W. BETH.

A REPLY TO MR. FOSTER.

IN the July issue of MIND¹ Mr. Foster argues that the interpretation I have offered elsewhere² of Plato's comparison between the constitutions of the state and of the soul is inconsistent with the political arrangements advocated in the *Republic*. He seems to reject the opinion that Plato ever thought 'that the souls of men differ so radically from one another that a member of any one of the three classes possesses only a single element of the three into which Plato divides the soul',³ and indeed says that I 'attribute' to him the opinion that Plato thought it, as if he meant that the opinion was never really his. But the passages I quoted⁴ from his book *Political Philosophies of Plato and Hegel* seem to me to justify my attribution, and where he now admits 'forcing Plato's psychological teaching too much into conformity with his political doctrine',⁵ I think he acknowledges as much. Since the *Note*⁶ in my book was chiefly directed to refuting this opinion, I am very glad to have his admission that he does not hold it.

But he still maintains that Plato's political theory has implications inconsistent with his doctrine of the soul. I allowed that the theory was not required by that doctrine; but my interpretation of the doctrine claimed to be consistent with the theory. Mr. Foster says that 'the result is an improvement on Plato, but it is not Plato.'⁷

Since he explicitly charges Plato with inconsistency, I do not see why my interpretation, though it were inconsistent with Plato's political theory, might not still be what he meant; but, indeed, I find Mr. Foster's arguments for the inconsistency quite unconvincing. And I certainly make no claim to have improved on Plato, but only to bring out his meaning.

'To possess the faculty of Reason in the soul is to be capable of Wisdom, to possess the Spirited Element is to be capable of Courage (Andreia). If all men's souls contain these elements, all are capable of these virtues.'⁸ Granted. 'On the other hand, these virtues are what qualify a man to be a ruler; furthermore, they are the sole qualifications which a ruler requires.'⁹ Not granted. A ruler needs temperance as well; and a man may have wisdom

¹ Pp. 350-354.

² P. 350.

³ P. 351.

⁴ P. 351.

⁵ *Essays in Ancient and Modern Philosophy*, iv.

⁶ *Op. cit.*, pp. 114-115.

⁷ *Op. cit.*, pp. 114-121.

⁸ P. 351.

⁹ *Ibid.*

without being wise enough to rule the state, courage without being fit to lead a forlorn hope. The wisdom required to rule the state is that ἢ οὐχ ὑπὲρ τῶν ἐν τῇ πόλει τινὸς βουλευσεται, ἀλλ' ὑπὲρ αὐτῆς ὅλης.¹ Those words distinguish it from the wisdom which takes counsel for the individual. Mr. Foster argues as if whoever has this last must have the other. He speaks of it as an 'obvious objection' to my view, 'that Plato undeniably thinks wisdom to be the same virtue whether it is exercised morally or politically, and that, therefore, he could have no ground, if he attributes moral wisdom to all, for confining political wisdom to a few'.² Here by political wisdom he means that which, in planning the state's order, would provide, say, for so many farmers and so many bricklayers; by moral wisdom that which in planning a farmer's or a bricklayer's life would fit his other activities to the demands of his calling. Is it true that, if a farmer has wisdom enough to do the second, he must have enough to do the first? If a Russian to-day is intelligent enough, when sent to help build a factory, to order his life so as to be a good bricklayer, must he be fit to share in the work of *Gosplan*? Mr. Foster cites *Rep.* IX., 590 c, d, where it is said that the mechanic should be δοῦλος τοῦ βελτίστου, and that if not possessing in himself the θεῖον καὶ φρόνιμον, he should be ruled by one who has it. But does he think this means that every detail in the ordering of a man's life must be prescribed for him? As 590 c shows, a man may be so ruled through laws which he does not make. In Plato's best state there may be only one φύλαξ. Could one man exercise all the intelligence and wisdom needed for ordering all citizens' lives? And if not, and wisdom may be shown in the humble sphere of any private life, must we therefore hold that whoever so shows it is fit to rule the state?

Mr. Foster says that Wisdom and Courage are for Plato the product of education, and of the education prescribed in the *Republic*; therefore, 'if he is to attribute to each member of the Third Class the Wisdom and Courage necessary for the ordering of his own life, Mr. Joseph must suppose the members of the Third Class to share the Guardians' education'.³ Not at all, any more than, because the temperance required of the Guardians is for Plato the product of the education prescribed in the *Republic*, Mr. Foster, who does not deny that each member of the third class is to have temperance, must suppose them to share the Guardians' education. It is the wisdom needed for ruling, the courage needed for upholding the order of the state against every foe without or within, that require this special education. It is true that almost nothing is said about the upbringing of the δημιουργοί. But does it follow Plato overlooked how they might be subject to influences moulding them to such wisdom and courage as their lives would need? Has not that happened in some communities throughout recorded time,

¹ *Ref.* iv. 428, c. 12.² P. 352.³ P. 353.

where often there was no schooling but such as family and community and work afford?

'If each subject of the state is to have scope for the exercise of individual judgement in his private life, there must be a limit set to the extent to which it is right for government to direct his conduct. But Plato sets no such limit to the ruler's authority.'¹ There are limits to what the ruler *can* do in ordering private lives; individuals must do much of the detailed ordering themselves; Aristotle noted the freedom of slaves in this respect. Therefore individuals will have need of wisdom, even under the most meticulous of paternal governments. There is no inconsistency between insisting on this, and abstaining from the folly of trying to prescribe in advance limits to the action which a paternal government may take in the interest of the community.

We may wish that so wise a man as Plato had been more friendly to democratic institutions. We may doubt whether statesmanship can be taught as he hoped it could, we may agree with Protagoras when he suggests, under the form of a myth, that aptitude for advising on the direction of public affairs is common to us all. We may believe that Plato erroneously supposed his political theory to find support in his psychological doctrines, and in that identity of constitution which he detected in the lives of a state and of a man. But his account of that identity, into which I do not think I have foisted anything foreign to his thought, seems to me perfectly consistent with his political doctrine, just because its truth is independent of the factors which make different political institutions suitable to different communities.

H. W. B. JOSEPH.

¹ P. 354.

ARE THERE 'DEGREES' OF THE MORAL EMOTION?

It is generally agreed that the apprehension of the moral 'ought', of duty, is accompanied by a specific kind of emotion. This emotion it is customary, following Kant, to designate by such terms as 'reverence' or 'respect'. We speak of action from the purely moral motive as inspired by 'respect for duty', or 'reverence for the moral law'. These terms, indeed, are not altogether satisfactory. Nothing is more evident about the emotion felt towards the moral ought than its qualitative uniqueness; and terms like respect and reverence, which have a fairly extensive non-moral employment, hardly do justice to that uniqueness. But a more suitable name does not seem available: and so long as it is understood that the relationship between moral respect and all varieties of non-moral respect is one of analogy rather than of species within a genus, the common usage need not mislead us.

So much, I think, is common ground amongst almost all ethical writers. It also appears to be common ground that this moral emotion is susceptible of degrees of intensity. This has been not so much explicitly maintained as tacitly assumed. That 'respect for duty' is an emotion which may be felt more or less strongly as between different persons, or as between different generations of persons, or as between different phases in the mental history of a single person—all this has seemed too obvious to call in question. And it certainly finds abundant support in the psychological implications of our common speech.

Yet I am inclined to believe that, if the question is raised, it will yield rather surprising results. There seems to me to be good reason for holding that the moral emotion cannot vary intensively at all, that it is quantitatively as well as qualitatively a 'constant'. In what follows I propose to argue the case for this view.

We shall best begin, I think, by considering some emotion which is beyond question capable of varying in degree, and enquiring as to the grounds of its variation. Let us take pity. Pity is an emotion evoked by the apprehension of calamity occurring to other beings in whose welfare we feel a sympathetic interest. Its stimulus is a complex situation, the constituents of which are subject to wide variation. And it seems evident at once that at least the primary cause of the variation of intensity in the emotion of pity is the variation of the constituents of its stimulus. The calamity may be

great or slight, its victims may be close friends or casual acquaintances, and in accordance with these and other like variations in the stimulus our pity is strong or mild.

The same holds good, I think, for all our ordinary human emotions. If we are asked why, in a given situation, we feel so very angry, or so very fearful, the natural reply is to point to certain characteristics in the constituents of the stimulus which explain the high intensity of the emotion we feel.

But let us turn now to the moral emotion. The moral emotion is evoked by apprehension of the moral ought. But the moral ought is a 'simple'. It is not resolvable into constituents, much less into 'variable' constituents. Nor can there be variations of degree in 'oughtness' itself. Moral oughtness cannot be more or less of a moral oughtness, but just either is or is not moral oughtness. And if this is so (I shall defend the view later against one possible line of attack) it would seem that the emotion evoked by apprehension of the moral ought cannot vary in intensity by reason of its external stimulus at any rate.

Our ordinary emotions may, however, vary in intensity in response to *internal* conditions as well as to *external* stimuli. What are we to say of this possible source of variation in the moral emotion?

These internal conditions may be classified sufficiently for our purpose under two heads. (1) Conditions which vary within a single person—such as the condition of one's liver or 'nerves', which may induce variations in the intensity of anger or fear in spite of identical external stimuli. (2) Conditions which vary as between persons, but are substantially invariable in the life of a single person. Under this head come any 'constitutional' tendencies to feel particular emotions with unusual strength or unusual weakness.

As regards (1), I do not find that any evidence has ever been advanced for the view—nor even that anyone has ever held the view—that the moral emotion can vary with varying conditions of body or mind, as such emotions as fear and anger obviously can. It hardly seems worth while, therefore, to discuss this abstract possibility. But it would be, I think, more plausible to assert variant internal conditions of the *second* class—if only because such an assertion is much less amenable to either proof or disproof by experimental evidence. The natural argument in its favour would be one based on analogy. If the intensity of the emotion of fear, or anger, or sex may vary according to the congenital constitution of the subject, why should not the congenital constitution of the subject induce variation in the moral emotion likewise?

I think there is a very good reason why not, a reason which becomes apparent when we appreciate the vital difference in the relationship between emotion and idea in the respective cases. In the case of fear (and the same seems true of all our ordinary human emotions) that relationship is relatively external. It is in principle possible to conceive the idea without conceiving the emotion: even

if (as is by no means certain) the two are inseparable in fact. We can in principle think of the awareness of danger existing without there existing also the emotion of fear. We do, of course, *expect* the awareness of danger to generate the emotion of fear as a matter of *fact*, on account of the empirically known constitution of the normal human subject. But my point is that this expectation is not guaranteed by *the idea itself*. Now in the case of the moral emotion it is quite otherwise. Here idea and emotion are intrinsically inseparable. Here it is *not* in principle possible to conceive the idea without conceiving the emotion. We cannot think of an *awareness* of the moral ought which is not at the same time the *feeling* of the appropriate *emotion* towards the moral ought. The relationship between the two is indeed far too intimate to be described in the language of stimulus and response, which we were hitherto content to use. In this case the idea involves the emotion by its intrinsic nature.

This, I think, was a large part of what Kant meant when he urged that respect for duty, though a feeling, differed from other feelings in that it was 'self-wrought by a rational concept'.¹ We may not agree with all that is implied for Kant by this phrase. But its main point, I think, is sound, *viz.*, that the moral emotion is intrinsically bound up with the moral idea itself, and does not imply for its explanation, as ordinary human emotions do, an additional reference to the particular constitution of the experiencing subject.

This seems to me an adequate reason for rejecting the argument based on the analogy between moral emotion and other human emotions. The uniquely intimate dependence of the moral emotion upon the moral idea, and its consequent independence of the particular constitution of the experiencing subject, causes the analogy to break down just at the point at which it would be serviceable for the argument. The conclusion to which our analysis points is that if the moral idea, the idea of the moral ought, does not vary, the moral feeling, since it is just the emotional aspect of that idea, cannot vary either.

But is it really the case, it may be asked, that the moral ought, as apprehended, is not capable of variation in intensity? This brings us to a new set of considerations. I have so far done no more than appeal to the reader's own moral experience to support the contention that it is strictly meaningless to speak of *more* or *less* of moral oughtness. But it is undeniable that we do all of us use forms of speech which seem to imply that we find meaning in that phrase. When we say, as we often do, that we apprehend act A to be a 'stronger obligation' than act B, what do we mean if we do not mean that the 'ought' which we apprehend in A presents itself with

¹ *Grundlegung zur Metaphysik der Sitten*, p. 22, footnote 2 (Rosenkranz edition).

a greater degree of oughtness than the 'ought' that we apprehend in B?

These expressions are entitled to careful consideration. Any theory which hopes to establish the view that the moral emotion cannot vary in intensity is obliged to try to 'explain them away' satisfactorily. It must try to show that the kind of experiences to which these expressions refer are not really the kind of experiences of which they are a strict verbal description: that the expressions are, in fact, loose and misleading forms of speech.

This I shall attempt now. I shall endeavour to show what it is that people really mean when they speak of apprehending a 'stronger' obligation to act A than to act B.

We may consider first the case in which A and B are mutually exclusive acts in a particular given situation. I may, for example, in a given situation, intelligibly say that I apprehend a stronger obligation to tell a lie, and thus prevent great mental distress in a critically ill patient, than to tell the truth. Here it is, I think, pretty clear what is really meant by 'stronger obligation'. Strictly speaking, I apprehend in this situation only *one* moral obligation, *viz.*, to tell the lie—for obviously we cannot believe that we ought to do two mutually exclusive acts: as Butler long ago pointed out, to be under two contrary obligations is equivalent to being under none at all—but I speak of it as the 'stronger' obligation (and thus seem to imply that telling the truth is *also* an obligation, though a weaker one) because I recognise that in all *ordinary* circumstances telling the truth is *also* a moral obligation. In the given situation, it is perfectly clear, telling the truth is not apprehended as having *less* moral oughtness than telling the lie, but just as having *no* moral oughtness at all.

We may also, however, speak of stronger and weaker obligations when A and B are not particular acts contemplated in a given situation, but general *types* of acts. We may say, perhaps, that we recognise justice as a 'stronger' obligation than charity. What do we mean when we speak thus? I suggest that our meaning is as follows. We recognise that both justice and charity ought, as a rule, to be ensued: but we recognise also that, since justice on the whole conduces more powerfully than charity does to the ultimate end or ends from which both derive their moral authority, we ought, should conflict ever occur between these 'virtues', to be 'just' rather than 'charitable'. In other words, we do not really, when we use this form of speech, apprehend a greater degree of oughtness intrinsically inhering in justice. What we apprehend is a greater degree of conduciveness to the ultimate end or ends from whose moral authority both justice and charity derive the moral authority they may have.

This interpretation would, of course, be denied by the Intuitionalist. He would say that both justice and charity are apprehended as possessing 'oughtness' in their own right: and some Intuitionists, alive to the problem of conflicting obligations, would go on to say

that one of them is recognised—presumably as a result of direct comparison—to possess a more 'stringent' oughtness than the other. I am not able, for many reasons, to accept the Intuitionist account of our moral experience. But in the present connection it will perhaps suffice to say this. The above doctrine implies that, when I bring charity into comparison with justice, I recognise that charity must sometimes 'defer' to justice, *i.e.* that charity is not *always* right. This can only mean that, whatever I may have previously believed to be the case, I now recognise that charity is not an unconditional obligation. I do not now apprehend moral oughtness as an inherent characteristic of the class of acts we call 'charitable'. But if so, there is no question of the apprehended oughtness of the one class of acts having 'more oughtness' than the apprehended oughtness of the other class. For only one of the two classes *has* apprehended oughtness.

I venture to cite one further illustration which, while it raises no new point of principle, may help to confirm what may still seem a somewhat paradoxical thesis. We should probably all say that we recognise a stronger obligation to attempt, at personal risk, to save the lives of 100 men than to save the life of a single man. Our real meaning, I have been suggesting, is that while we recognise that normally both are things that we ought to do, we also recognise that saving the group is more conducive to the ultimate moral end than saving the single man, and that, accordingly, if a situation arose in which the two lines of action were mutually exclusive, we ought in preference to try to save the group. If that is our meaning, it is quite intelligible to express it by saying that we recognise the obligation to try to save the group as the 'stronger' obligation. It would even be intelligible to say that we recognise it as 'a hundred times stronger'. But it would be quite unintelligible, I submit, to speak of the obligation as a hundred times stronger, if we mean by that that it presents itself with a hundred times the intrinsic oughtness of the obligation to try to save the life of the single man.

There are, of course, other forms of speech that are *prima facie* indicative of degrees of the moral emotion, or of degrees of moral oughtness. But there are none, so far as I can see, which are not capable of fairly easy resolution along lines similar to those set out above. Kant himself has given countenance to the view against which I am arguing in more than one passage, notably in the passage where he urges us to the 'cultivation' of the moral feeling 'and the strengthening of it even by admiration of its inscrutable origin'.¹ But his mistake is, I think, obvious. The 'admiration' of the 'inscrutable origin' of the moral feeling will, no doubt, enhance the emotional colouring of our apprehension of duty. But it will do so not by intensifying the specifically moral

¹ Preface to the *Metaphysische Anfangsgründe der Tugendlehre*, p. 247. (Rosenkranz' edition).

emotion but by adding a new emotion. The reason why it seems plausible to speak of this new emotional factor as 'strengthening' the moral feeling is just that there is a natural disposition to identify 'inscrutable origin' with 'Divine origin', and the new emotional factor therefore tends to *support* the moral emotion by lending something in the nature of a religious sanction to moral conduct. The 'strengthening' of the moral feeling is, it is clear, a strengthening by external support, not by inherent intensification.

In conclusion, it seems proper to add a word or two about the philosophical significance of the issue we have been considering: for at first glance the matter may appear a somewhat minor one, hardly worthy of so prolonged an investigation. As I see it, however, our problem has a vital bearing upon the interpretation of what is perhaps the crucial situation in the Libertarian-Determinist controversy—the situation in which the agent seems to himself to be 'rising to duty' in defiance of his strongest felt desires. The Determinist holds that this act is fully explainable in terms of antecedent emotions and conations; but he has considerable difficulty in working out his explanation in detail. McDougall's explanation is well-known, that the 'weaker' desire is reinforced by a new impulse excited from within the self-regarding sentiment. But apart from certain other grave difficulties (which I have pointed out elsewhere), this solution is defective by reason of its limited application: since we certainly *sometimes* have the experience we call 'rising to duty' in 'the line of greater resistance' when the most careful introspection reveals no trace whatsoever of impulses from the self-regarding sentiment. It then occurs to one to ask, may we not, perhaps, in those cases where the self-regarding sentiment is inoperative, appeal to the moral emotion of 'respect for duty' to furnish the new reinforcing impulse? It is in this connection that the question of the capacity of the moral emotion (and its corresponding 'impulse') to vary in intensity will be found to be of great moment. A decision one way or the other will not solve the free will problem. But it will contribute very substantially, I think, to its more profitable discussion.

C. A. CAMPBELL.

ON THE AXIOM OF REDUCIBILITY.

The theory of types, as presented in *P.M.*,¹ falls into two parts: one is concerned with the purely logical antinomies, and the other with the semantic ones.² The first part classifies propositional functions into a hierarchy of *types*; the second part superimposes a further classification into a hierarchy of *orders*, such that functions may differ as to order even though they be of the same type—indeed, even though they be formally equivalent, *i.e.*, satisfied by just the same arguments. To remove an intolerable restriction which the second part of the theory imposes upon logic and mathematics, the *axiom of reducibility* is adopted in *P.M.*³ This axiom moderates the second part of the theory by asserting that for every propositional function there is a formally equivalent one which is *predicative*, *i.e.*, has the lowest order compatible with its type.

In his preface to the second edition of *P.M.*⁴ Russell adopts a partial extensionality principle for propositional functions, to the extent of identifying functions which are formally equivalent and of the same order. He continues to hold, however, that formally equivalent functions may differ in order and thereby fail of identity.

We are given a criterion for determining whether two expressions denote propositional functions of the same order;⁵ we ask, namely, whether the expressions differ in a certain specified notational respect (which need not be set forth here). Discrimination between formally equivalent propositional functions of different orders is thus grounded, to the extent at least of there being a difference in the expressions which denote them. Still, dissimilar expressions frequently denote the same thing—indeed, the partial extensionality principle shows that expressions can differ in some respects and still denote the same propositional function; hence the partitioning of formally equivalent functions into orders might still be viewed as a manufactured distinction of subject matter, though the notational criterion adopted is objective. But in any case the authors of *P.M.* believed that the partitioning was needed.

¹ Whitehead and Russell, *Principia Mathematica*, vol. i., pp. 49-54, 161-165.

² See Ramsey, *Foundations of Mathematics*, pp. 20, 24-25.

³ Pp. 55-60, 166-167.

⁴ P. xxxix.

⁵ Rather than depart from the attitude of *P.M.* in respects inessential to present concerns, I here retain the realistic idiom. Certain ideal objects called propositional functions are reckoned to the subject matter of *P.M.*, and these are denoted by expressions occurring in the book.

Ramsey¹ has argued against the need of the partition, and advocated abandonment of the second part of the theory of types, together with its antidote the axiom of reducibility. Some subsequent logicians have followed this course.² The purpose of the present note is to offer a different and more formal consideration in support of Ramsey's standpoint; namely, to point out that by the very adoption of the axiom of reducibility and the partial extensionality principle we grant the validity of the system which Ramsey urges: the system of *P.M.* minus the second part of the theory of types. This is seen as follows.³

By the axiom of reducibility, there is for every propositional function a formally equivalent predicative one. Moreover, there is only one: for two such would be formally equivalent and of the same order, and hence identical by the partial extensionality principle. We may therefore speak of *the* predicative function which is formally equivalent to a given function. Then let us construct another system, differing from that of *P.M.* only in this interpretational respect: every expression which for *P.M.* would denote a non-predicative function ϕ is to be construed in the new system as denoting rather the predicative function which is formally equivalent to ϕ . The subject matter of the new system comprises none but predicative functions; all partitioning of functions into orders as distinct from types disappears, and the system is indistinguishable from what that of *P.M.* would have been if the distinction of orders had not been invented.

Granted the partial extensionality principle, the above argument shows that either the axiom of reducibility is not legitimate to begin with, or else both it and the second part of the theory of types are superfluous.

There is, however, a metamathematical or syntactical application in which analogues of the hierarchy of orders and the axiom of reducibility are of less doubtful significance. Let us classify, not the propositional functions, but their symbolic expressions; these will be distinguished in point of expressional order if they differ in the notational respects which, from the earlier standpoint, constituted a difference of order in the functions expressed. We next derive the notion of predicative expression, in analogy to the earlier notion of predicative function. Finally, we can frame a metamathematical analogue of the axiom of reducibility, in application to the syntax of a given formal system *S*. Roughly, this would state that for every expression *E* of *S*, of the kind used for expressing specific propositional functions, there is a predicative expression *F* of *S*

¹ *Op. cit.*, pp. 20-21, 28-29.

² See Carnap, *Abriss der Logistik*, p. 21; Church, "Quine on Logistic," *Bull. Amer. Math. Soc.*, vol. 41 (1935), pp. 601-602.

³ This argument was included in my doctoral dissertation, *The Logic of Sequences* (1932; manuscript in Harvard Library), pp. 5-8.

such that a statement of formal equivalence connecting the expressions E and F is a theorem of S . In this form the axiom affirms a certain syntactical property of the system S ; an important property, but one which is lacked by most comprehensive systems, including that of $P.M.$ ¹ These analogues of the orders and the axiom of reducibility of $P.M.$ deal with a metamathematical subject matter of expressions, however, and not with the subject matter of $P.M.$

W. V. QUINE.

¹ For an example of reducibility in this syntactical sense see my "Definition of Substitution," *Bull. Amer. Math. Soc.*, vol. 42 (1936), particularly the footnote.

V.—CRITICAL NOTICES.

Wahrscheinlichkeitslehre. By HANS REICHENBACH. Leiden: A. W. SIJTHOFF'S UITGEVERSMIJ, 1935. Pp. ix + 451. Fl. 11.50.

PROF. REICHENBACH, now of the University of Istanbul, is the outstanding member of the "Berlin group" of thinkers devoted to analytic philosophy. Closely related intellectually to the *Wiener Kreis*, this group has been concerned less with the solution or dissolution of traditional problems than with the analysis of concrete methodological difficulties in the exact sciences. Prof. Reichenbach has already given us a series of splendid volumes on the epistemological and methodological principles of relativity theory. The analysis of the meaning of "probability" has been, however, his deepest concern; since 1915, when he was still under the influence of Kant, he has published a large number of papers on various phases of the subject. The present work offers, therefore, the conclusions of twenty years of labor. It aims to provide not only a systematic account of the mathematical and methodological foundations of the probability calculus, but also a definitive solution of the problem of induction as raised by Hume; and it contains what is to-date undoubtedly the most complete and ingenious defence of the "truth-frequency" interpretation of probability statements. It is without question of first-rate importance to all students of applied logic.

The book falls into ten sections and a mathematical appendix, the first eight of which, excepting the introductory one, are devoted to the mathematical elaboration of an abstract, uninterpreted set of postulates for the calculus of probability. Since these sections are chiefly of mathematical interest, only the points relevant to the methodological discussions will be noticed. But I cannot help expressing regret that Prof. Reichenbach did not apportion his available space in favor of a more detailed working out of his views on verification and induction; these latter are often sketchily portrayed, especially in contrast with the mathematical portions of the book, and can frequently be judged simply as programs for further elaboration by him.

In the introductory section, Prof. Reichenbach maintains that judgments of probability occur in all activities of daily life and science, and that the meaning of such judgments in the latter are continuous with the meanings in the former. However, he prefers to take as his point of departure the probability statements of the

sciences, on the ground that their meaning and logical structure are more evident in this context. It seems clear, therefore, that Prof. Reichenbach wishes to clarify the meanings which such statements do in fact have, and not to *propose* a meaning which they should have. On the other hand, he commits himself at the outset to the view that the term "probability" has a univocal sense in every context (p. 5). This is innocent enough as a hypothesis, but is not evident *prima facie*, and contemporary writers are certainly not in agreement about it. Consequently, it is an assumption which needs to be tested at the close of the proposed analysis of the meaning of probability statements. Prof. Reichenbach makes some attempt to do so, with a success which remains to be estimated, though not, I think, with a full consciousness of the possibility of an alternative view. But in any case, it is this assumption which guides the development of his argument: he constructs an abstract calculus of "probability", then offers an interpretation of the free variable "probability implication" which satisfies the postulates, and finally urges that this interpretation covers the sense of probability statements wherever they occur.

After a compact account of the apparatus of modern logic in Section Two, Prof. Reichenbach expounds the formal or abstract theory of the probability calculus in Section Three. "Probability" is the name of a quantitative tryadic relation, symbolized by $\overset{p}{\Rightarrow}$,

which holds between two classes and an ordered pair of correlated elements taken from each of two series. Accordingly, probability statements are analyzed to have the form $(x_i \in O \overset{p}{\Rightarrow} y_i \in P)$, which

may be read: for all correlated pairs $x_i y_i$, x_i is a member of O implies with degree of probability p that y_i is a member of P. (The abbreviated forms $(O \overset{p}{\Rightarrow} P)$ and $W(O, P) = p$ are also used.) Prof.

Reichenbach gives several illustrations of such statements (p. 56 ff.). The relation " $\overset{p}{\Rightarrow}$ " is further defined implicitly, by the condition

that it "satisfy" five sets of abstract postulates. Only the first four of these are stated in the present section, which is restricted to the "elementary" probability calculus. These postulates, together with certain rules of operation, are shown to be sufficient for deducing the usual theorems of the calculus.

However, some remarks which Prof. Reichenbach makes concerning one of the postulates are worth noting. This postulate is: $(O \supset P) \supset (O \overset{p}{\Rightarrow} P)$ ($p = 1$); or in words, if x_i is an O formally implies (in Russell's sense) that y_i is a P, then the probability is 1 that y_i is a P when x_i is an O. He points out, in the first place, that the converse does not hold, because "Aus der Wahrscheinlichkeit 1 folgt die Gewissheit nicht. Gewissheit und Wahrscheinlichkeit 1 stehen vielmehr im Verhältnis des engeren zum weiteren Begriff; die Gewissheit ist ein Spezialfall der Wahrscheinlichkeit 1" (p. 65). This is an unsatisfactory way of putting the matter,

since Prof. Reichenbach wishes to regard probability as an objective relation having no reference whatsoever to anyone's state of mind (e.g. p. 331). Consistently with his views, therefore, he ought to regard a probability of 1 and certainty (which ordinarily does involve a reference to a state of belief) as incomparable terms. In the second place, Prof. Reichenbach seems to hold that probability implication is of the same "logical type" as Russell's formal implication (p. 66). This is not a helpful comment, since the calculus of formal implication is *extensional* while that of probability implication is not; moreover, upon the interpretation which he assigns to \Rightarrow , probability implication seems to be a semantic and syntactic characterization of relations between statements, and not, as in the case of formal implication, between what is *signified* by statements. (Incidentally, Prof. Reichenbach's discussion of material, formal, and tautologous implication (p. 33), and of the relation of modal distinctions to truth and falsity (p. 378) suffers from the same unclarity.)

The interpretation which Prof. Reichenbach assigns to the implicitly defined "probability implication" is as follows: $\frac{1}{n} \sum_{i=1}^n \chi(x_i \in O)$ is the number of elements x_i which satisfy $x_i \in O$, where the subscripts are numbered from 1 to n , with a similar notation for more complex propositional functions. Then

$$H_n(O, P) = \frac{\frac{1}{n} \sum_{i=1}^n \chi(x_i \in O)(y_i \in P)}{\frac{1}{n} \sum_{i=1}^n \chi(x_i \in O)},$$

is the relative frequency with which the y_i 's are members of P when the correlated x_i 's belong to O . If, for a sequence of ordered pairs $x_i y_i$, H_n approaches a limit p as n increases without limit, p is the probability within this sequence that a y is a P when its correlated x is an O ; i.e.

$$W(O, P) = p = \lim_{n \rightarrow \infty} H_n(O, P).$$

Such a statistical interpretation of probability has of course been proposed before, both in Europe and America. Prof. Reichenbach's formulation differs from that of most of his predecessors in two important respects: the probability relation is first defined implicitly by a set of postulates and only subsequently interpreted; and the sole material requirement for sequences is that they satisfy the condition of converging toward a limit in the above specified sense. In both respects, for example, his formulation has obvious methodological advantages over the theory of Von Mises, who requires that sequences also satisfy the condition of being "completely random".

The development of the next five sections, however, in no way depends on this interpretation. In Section Four, with the aid of

one further set of postulates, theorems on the internal structure of infinite sequences are proved, and an important distinction between "normal" and "anormal" series is defined. Normal series possess a certain type of irregularity or randomness in the succession of their terms, which may be characterized quite formally as well as on the basis of the statistical interpretation of probability. One point of general interest emerges. Prof. Reichenbach's definition of normality is much weaker than that of Von Mises, whose treatise *Wahrscheinlichkeitsrechnung* is a standard work in this field. According to Von Mises, a sequence is normal only if the limit with which a property P occurs in every "preassigned" sub-class in a series (i.e. selected without reference to P) is identical with the limit in the main series; while Prof. Reichenbach defines a series as normal when the limits of only certain kinds of preassigned sub-classes coincide with the limit of the main series. Now of course, as Prof. Reichenbach admits, which kind of series shall be defined as "normal" is arbitrary. Nevertheless, it follows that no normal series in Von Mises' sense can be characterized by any mathematical rule of formation, while Prof. Reichenbach's normal series can, though he himself does not offer any mathematical models. Prof. Reichenbach's definition has therefore an undoubted practical advantage. However, his criticism of Von Mises is made not only on this last ground, but on the further ground that the latter's definition of "normality" is meaningless (kaum sinnvoll, p. 142), because no mathematically definable models can be given for it. This further ground, however, is dubious, and I suspect that Prof. Reichenbach has been taken in by the Brouwerian thesis that mathematical "objects" must be "constructible". On another occasion (*Mathem. Zeitschrift*, Bd. 34, p. 594), he himself has formulated Von Mises' definition of normality as follows: $(X)\{(X \text{ is not a function of } P) \supset [W(O, X, P) = W(O, P)]\}$, where X is any preassigned sub-class of O. I see no good reason why this postulate may not be included in the premisses of a deductive theory as well as any other, unless indeed one dogmatically excludes it on the ground that sequences which would satisfy it are not "constructible". On that ground, however, as is well known, large portions of modern pure mathematics would also have to be excluded.

In Section Five a good account is given of the usual auxiliary concepts employed in theoretical statistics, while in Section Six "continuous" or "geometrical" probabilities are considered. Section Seven investigates the statistical properties of probability sequences, and Bernoulli's theorem is proved for normal series. Prof. Reichenbach gives an admirably clear statement of just what this theorem does assert, which should put to an end, once and for all, repeated attempts to use the theorem for deducing a frequency interpretation of probability from a non-frequency reading of it.

Section Eight develops the mathematics of probabilities of "higher order". Though brief, this section is very important for later discussions. As will be seen presently, on Prof. Reichenbach's

view a statement of probability is itself only probable. Thus if the statement "P implies Q with probability q " has a probability of p with respect to O, then p is a probability of *second order*; and so on, for other orders. However, since on the frequency view the attribution of a probability to a *single* statement is an elliptic way of referring to the relative frequency in a series of statements, the study of second and higher order probabilities leads to the consideration of series of series of elements. There are of course no mathematical novelties in this problem. The theorem of greatest importance for Prof. Reichenbach's theory of induction is: If in the first n terms of a probability series (i.e., one for which the relative frequency of a character P has a limit) the relative frequency of P is h , then the probability that beyond the first n terms the relative frequency of P will remain in the interval $h \pm \delta$ approaches unity as n increases without limit (p. 321).

After this long mathematical preparation, Prof. Reichenbach finally turns to the problems which face the application of the calculus of probability to empirical matters of fact. Two types of questions are distinguished under the general head of *Das Anwendungsproblem*: (1) the problem of the *meaning* of statements of probability, and (2) the problem of the *validity* of such statements (p. 325).

(1) Persuaded of the univocality of "probability", Prof. Reichenbach tries to show that *all* statements containing the term can be correctly translated into statements involving limits of relative frequencies. He finds that the common element in the objections to this view lies in the claim that probability judgments can be made about *single* "events" or statements. And I think he is right in holding that if it were the case that a single statement (say a general statement or theory) could without ellipsis be said to be "probable" on given evidence, the meaning associated with the word could not be rendered by the "truth-frequency" interpretation. He must therefore show that all such alleged probabilities of single statements are in fact elliptical.

It seems to me that Prof. Reichenbach makes out a strong case for the position that *some* statements of probability (e.g., in games of chance, actuarial work, law courts, history, many branches of physics, etc.) are to be analyzed in the way he proposes. The main reason for believing this is so, briefly, is that it is on the basis of statistical considerations that we decide the correctness of the probability statements we make in such contexts. Moreover, in these cases, probability statements about single events could not possibly be verified even partially, since the probability estimate we make of any one future event is compatible with any mode of realization of that event. And since "*etwas meinen* heisst, sich erlebbare Konsequenzen ausdenken, die eine Aussage bestätigen würden und verschieden sind von den ebenfalls erlebbaren Konsequenzen, die die Aussage widerlegen würden", (p. 330) Prof. Reichenbach concludes that the conception of the probability of a single statement is meaningless.

This conclusion, however, seems to me dubious. If "probability" does mean relative frequency, then indeed every statement of the probability of a single event is either elliptical or meaningless. But if "probability" is not a univocal term, if, for example, it is sometimes used to refer, as some hold, to the degree to which a statement has been tested, it surely could be used correctly and significantly to refer to single statements without ellipsis.

Perhaps the most important and obvious case in which "probability" is used in connection with a single statement, is when we talk of the probability of a scientific theory—e.g., when we say that on the basis of the experimental evidence the Newtonian theory was "more probable" at the time of Laplace than at the time of Newton. Are we here using "probable" in the sense which Prof. Reichenbach specifies? Though his own answer is in the decided affirmative, on this important point his discussion is woefully inadequate. His most explicit account of the matter is as follows: "Wir können auch die Theorie klassifizieren und damit in einen Bereich anderer Theorien einordnen, in welchem die relative Zahl der sich bewährenden Theorien gezählt werden kann. Zwar besteht eine wissenschaftliche Theorie selbst bereits aus Wahrscheinlichkeitsaussagen, sodass sich der Fall kompliziert; es handelt sich hier also um eine Wahrscheinlichkeit höherer Ordnung . . . Es besteht prinzipiell durchaus die Möglichkeit, die Wahrscheinlichkeit z.B. für das Zutreffen der physikalischen Quantentheorie zahlenmässig zu bestimmen, indem man sie mit anderen physikalischen Theorien ähnlicher Art in eine Klasse zusammenfasst und für diese die Wahrscheinlichkeit durch Auszählung bestimmt" (p. 329).

A somewhat fuller statement is given by him in reply to criticism of his views by Karl Popper. There are two ways, he there says, in which we may try to define the probability of a theory. "Zunächst kann man die Gesamtheit der experimentell prüfbaren Aussagen, die zu der Theorie gehören, auszählen und die relative Häufigkeit der zutreffenden Aussagen darunter zählen; diese relative Häufigkeit kann nun als Mass für die Wahrscheinlichkeit der Theorie betrachtet werden. Wir wollen sie hier als *Wahrscheinlichkeit erster Form* bezeichnen. Zweitens aber kann man auch die Theorie als ideologisches Gebilde in eine Klasse einordnen mit anderen ideologischen Gebilden ähnlicher Art, also mit anderen von Wissenschaftlern aufgestellten Theorien, und dann in dieser Klasse die relative Häufigkeit auszählen; wir wollen diese Wahrscheinlichkeit als *Wahrscheinlichkeit zweiter Form* bezeichnen". The procedure for both methods is next sketched for the quantum theory, though without the essential details which the matter requires. (*Erkenntnis*, Bd.V, p. 275).

There are serious difficulties which stand in the way of a ready acceptance of Prof. Reichenbach's suggestions, though I cannot do adequate justice in this notice to the issues involved. (a) According to his first definition, we are required to view every theory (i.e., any

statement containing unrestricted universal operators) as an infinite logical product, apparently of singular statements. While I do not know that this can *not* be done, neither Prof. Reichenbach nor anyone else has indicated satisfactorily how this part of his proposal is to be carried out. Until he has done so, the suggestion is a program and not an analysis of the meaning of probability in use. (b) Let us assume, however, that this difficulty can be overcome, and that Newtonian mechanics, for example, can be regarded as an infinite logical product. If now one-tenth of these conjuncts were experimentally false, the probability of the theory, on the first definition, would be 9/10. Assuming that *some* clear sense can be attached to the common phrase that the "probability of a theory is high", is the result just obtained in agreement with the usual meaning of such a phrase? While I am not certain of the answer, it seems to me that it is in the negative, for the reason that a theory whose predictions are false one-tenth of the time would not be entertained as satisfactory by any physicist, and would not be assigned the stated probability value. (c) With respect to the second proposed definition, a set of principles must be formulated for *unambiguously* classifying theories. Assuming that such principles could be stated, however, an infinite series or at least a very large number of similar theories would have to be produced, for each of which some material evidence must be presented. It would not be sufficient simply to produce a set of *possible* theories for which such evidence is lacking, since the whole point is to estimate the relative frequency with which theories of a certain type are found acceptable on empirical grounds. But if we turn to the history of science, we find that in fact there is only a small class of theories for comparison, so that it seems that for this practical reason alone the proposed definition will not carry us far. (d) There is, moreover, a serious logical difficulty. In the case of singular statements like "This coin will fall head when next tossed into the air", the events corresponding to them can be examined and the statements confirmed as true or as false. It is on that assumption that the probability of such statements are both *defined*, and *estimated* on the basis of empirical data. But with respect to a theory, belonging by hypothesis to a class of similar theories, how shall we define, to say nothing of estimate, its probability? For it is admitted that no theory in the class can be *known* to be true in the sense in which singular statements like the above can be known to be true. And if it be suggested that we need only know the *probabilities* of the theories, the reply is that on Prof. Reichenbach's view we cannot even *know* the degree of probability of a theory, and that in any case the entire process would then be circular. While Prof. Reichenbach makes an ingenious use of the notion of higher probabilities to circumvent this objection, I do not think his attempt is successful. (e) Finally, it is not altogether clear whether either of Prof. Reichenbach's proposed definitions is intended as an analysis of the meaning which the phrase "probability of a theory"

does in fact have, or as a *proposal* to attach a certain meaning to the phrase. If he intends the latter, the sole question is whether the proposal is feasible and acceptable to those who wish to use the phrase. There are some *prima facie* grounds, however, for supposing that neither of Prof. Reichenbach's definitions covers the common meaning of the phrase. In the first place, many physicists frankly admit that the notion of a theory being probable has no fixed, "objective", meaning for them; a careful search of scientific treatises reveals that the probability of theories is not discussed in them, and that in any case no procedures for calculating the probability of theories in the sense specified by Prof. Reichenbach are employed. In the second place, eminent men of science repeatedly assert that a theory is found satisfactory by them partly on esthetic grounds, partly because they know no alternative theory, and partly because the consequences of the theory have been tested in accordance with a definite technique. Without venturing at this place to offer an alternative analysis of the phrase in question, it seems to me that the last suggestion, thrown out but left undeveloped by Charles S. Peirce, comes closer to stating the procedures actually employed in the sciences than anything that Prof. Reichenbach has indicated on this matter. Although on Peirce's view the acceptance of a theory is not completely arbitrary, theories are not "probable" in the special frequency sense to which Prof. Reichenbach subscribes.

(2) There remains the second *Anwendungsproblem*: the problem of the validity of probability statements, when "probability" is interpreted in Prof. Reichenbach's sense. The central difficulty arises as follows: Since probability is defined as the limit of relative frequencies, the determination of an actual probability seems to entail either the knowledge of the law of the infinite series, or an examination of the terms of such a series *seriatim*. But except in the rare cases where the series is "intensionally given" (i.e., mathematically defined), the first alternative is impossible; while for an "extensionally given" (i.e., empirical) sequence, we can in fact examine no more than a finite segment of the series. Hence we are faced with the task of inferring the *limiting* value of relative frequencies from a finite number of such ratios for which we know no law of formation. On the other hand, if h^n is the relative frequency of some character in a series for the first n terms, the condition that the ratios converge to a limit p can be stated as follows, where δ is any small number: $(\delta)(\exists n_0)(n)[(n > n_0) \supset (h^n \subseteq p \pm \delta)]$. But since this statement contains both universal and particular quantifiers, neither it nor its contradictory is capable of being completely verified as true. Whatever hypothesis we may make about the limiting value of the ratios, it is not capable of being definitively decided either way; and indeed it is easy to see that *any* state of affairs is compatible with a given hypothesis. If, then, there can be no facts which would verify or refute statements of probability interpreted as Prof. Reichenbach interprets them, they are *unentscheidbar*,

and should be ruled out as meaningless by the principle of verifiability (p. 354). Nevertheless, in spite of this array of difficulties, in practice we do make statements of probability and distinguish between them on the basis of *empirical* evidence.

It might be thought, therefore, that a *weaker* condition than that of convergence to a *limit* must be imposed upon the sequences considered, though this condition would still fall within the general framework of a statistical interpretation of probability. But Prof. Reichenbach shows beyond much dispute that within the framework of the mathematical procedure he adopts, the limit notion or some equivalent is unavoidable. His own solution of the difficulty is that while probability statements are *unentscheidbar* in the traditional two-valued logic, we may adopt a more general logic in which propositions have a continuous scale of "truth-values" and in which probability statements are "inductively verifiable" (*induktiv entscheidbar*). To this proposed solution he devotes the tenth and final section of the book. It turns out, in brief, that instead of facts verifying or not verifying probability statements, they verify them, on Prof. Reichenbach's view, "more or less"; this simply means, according to him, that statements of probability are themselves only probable, that the "strict" two-valued logic must be replaced by a many-valued "probability-logic", and that the notion of "truth" must be viewed as a special case of "probability" (p. 360).

Prof. Reichenbach's first task, therefore, is to show the relevance of the many-valued logics to his conception of probability. The central idea is roughly as follows: If in the statement $(i)(z_i \in O \supset x_i \in P)$

we assume that z_i is compact with respect to O (this may be done without loss of generality), we may write $W(P) = p$ instead of $W(O, P) = p$; and if we use the notation ϕx_i instead of $x_i \in P$, it may finally be written as $W(\phi x_i) = p$. Now just as the function ϕx can be predicated of some *individual argument* x , to yield a proposition which has *two* possible truth-values, so Prof. Reichenbach proposes to predicate the function ϕx of an *ordered sequence* (x_i) to yield a sequence of propositions (ϕx_i) —called the *based propositional function* (*fundierte Satzfunktion*)—which has a *series* of possible "truth-values", its probability p . Prof. Reichenbach concludes that statements of probability are about the "truth-value" of a based propositional function (p. 376), and shows how the formalism of the many-valued logics is applicable to these infinitely numerous "truth-values". Since, moreover, every matter-of-fact statement can be affirmed only with some degree of probability, the two-valued scheme must be abandoned for this multi-valued logic and the notion of truth surrendered for that of probability.

While Prof. Reichenbach's scheme is an interesting attempt to find a generalization and application for multi-valued calculi, it is not easy to see why he attaches the importance to it that he does. (a) He himself recognises that any statement made within the framework of his infinitely-valued logic can be translated into statements

of the two-valued system without loss of logical content (e.g., p. 372). At best, therefore, using the former simply provides a more convenient mode of statement. (b) Moreover, since the definition of probability (i.e., the infinitely numerous possible "truth-values") is given in terms of two-valued distinctions between statements, the "strict logic", of truth and falsity is fundamental to Prof. Reichenbach's "generalized logic", so that the latter is simply a chapter in the former. (c) And finally, it is not clear in what sense he thinks he has replaced truth by probability. The adjective "true" in its usual meaning refers to a relation between a "proposition" and a "fact", even though the grounds upon which we assert a proposition as true may never be complete. In Prof. Reichenbach's usage, the term "probable" is not employed in this way, and statements of probability seem to be both semantical and syntactical. I think, therefore, that it is a serious mistake to regard "true" as a special case of "probable"—a mistake which may be encouraged by his convention of writing $W(P)$ for $W(O,P)$ when the sequence is compact with respect to O .

However that may be, it is in terms of the apparatus of his multi-valued logic that Prof. Reichenbach formulates his theory of "inductive verifiability". Its central idea is that of making a *wager* (Setzung). Since in the present we cannot verify as true or as false any statements involving the future, but since we must and do take practical attitudes toward them, Prof. Reichenbach believes that we can only wager (setzen) upon them. He distinguishes two procedures: the process of making *optimum wagers* (optimale Setzung), and that of *approximate wagers* (approximative Setzung).

Suppose we have two alternatives: "This die will fall six uppermost on the next toss", and "This die will not fall with six uppermost on the next toss". We do not at present know which is true, but nevertheless we might wager on them. Each wager, however, is subject to an *evaluation* (Beurteilung) because with each of them is correlated a probability—the probability, namely, which is associated with each of the above statements when these are taken to belong to a class of statements for which a probability, defined as above, exists. Every statement about the future is thus a wager. We make an *optimum wager* when we wager on an alternative with the greatest correlated probability. Moreover, if we wager on an alternative P , and if we can find a preassigned subclass of the conditions O such that $W(O,A,P) > W(O,P)$, we improve our chances of obtaining P by placing wagers on P only under the circumstances that both O and A are present. As Prof. Reichenbach points out, the sole justification for conducting optimum wagers is that if we continue to wager in this manner, we are bound to obtain the desired character P in the largest proportion of cases; and while a wager is neither true nor false nor even probable (since a single "event" has no probability), it is not arbitrary.

Prof. Reichenbach has here outlined a perfectly sound procedure,

well known to students of statistical methods, and repeatedly called to their attention in the literature of the subject. However, it is a possible procedure only if we know the evaluations of the wagers contemplated. Hence for the cases when we do not know them, Prof. Reichenbach offers the alternative method of approximate wagers. Since the evaluation of a wager entails the knowledge of a probability; since, for reasons already stated, a statement of probabilities can not be *known* to be true; and since such a statement is a singular statement which, strictly speaking, cannot be said to be probable, Prof. Reichenbach maintains that we must place a wager on it, obtaining a wager of the *second order*. Such second-order wagers are themselves evaluated by correlating them with second order probabilities, and so on indefinitely. There remains the problem, however, of stating the method for estimating the probabilities of the various orders in question, on the basis of a *finite* number of extensionally given ratios of frequencies—in short, the problem of induction.

This method is given by the following *Inductive Rule*: "Wenn ein Abschnitt von n Gliedern einer Folge x_i vorliegt und für P die Häufigkeit h^n besteht, und wenn ferner über die Wahrscheinlichkeit zweiter Stufe für das Eintreten eines bestimmten limes p nichts bekannt ist, so setzen wir, dass die Häufigkeit h^i ($i > n$) bei weiterer Verlängerung der Folge einem limes p innerhalb $h^n \pm \delta$ zustrebt" (p. 397). The point of the rule is that if the series *has* a limit, by advancing in the series and successively correcting the values of h^n for greater and greater values of n , we must finally hit upon an n in a finite number of steps at which place the series begins to converge. Although we never *know* that we have found such a place of convergence (Konvergenzstelle), we do know that by following the inductive rule the goal must finally be reached.

Prof. Reichenbach supplements the Inductive Rule by an account of a method for reaching a place of convergence more rapidly. For this purpose, second-order probabilities must be estimated in accordance with the above procedure, and then employed to evaluate the wager that the first order probability lies in a definite interval. In terms of these wagers of higher orders, to be evaluated by considering series of series of elements, Prof. Reichenbach believes that we may compare the evaluations of first-order wagers and correct them accordingly. But while the hierarchy of wagers is in principle infinite (for the evaluation of a wager of the m -th order cannot be known to be true and is itself simply an approximate wager), in practice the hierarchy is terminated after a finite number of orders. This is justified, Prof. Reichenbach claims, because the higher-order approximate wagers are independent of the individual first-order wagers, and because the convergence of the higher-order relative frequencies is in general more rapid than that of the lower-order ones. Consequently he maintains that while probability statements are *unentscheidbar* in a two-valued logic, they are "inductively verifiable" in the multi-valued logic (p. 406-7).

All these conclusions are obtained on the assumption that the series considered do have limiting relative frequencies, and the problem remains to find some grounds for that assumption and so to justify the use of the Inductive Rule. Prof. Reichenbach's answer is that the assumption cannot be proved, that the Rule is neither a logical necessity nor an empirical law, but that the rule formulates a method for discovering approximations to the limits *if* limits exist. Hence even in the absence of knowledge that they do exist, it is reasonable to use the Rule. If the series do not have limits, that fact will ultimately appear in the results obtained by following the Rule; on the other hand, if we can at all succeed in setting up hypotheses which enable us to predict, we shall succeed by using the Rule. Hence Prof. Reichenbach formulates what he regards as the definitive solution of the Humean problem: "Die Induktionsregel ist die günstigste Setzung, weil sie die einzige Setzung ist, von der wir wissen: wenn es überhaupt möglich ist, Zukunftsaussagen zu machen, so werden wir sie durch diese Setzung finden" (p. 418).

Prof. Reichenbach's resolution of the *Entscheidungsproblem* for statements of probability depends upon his view that theories are "probable" in his "truth-frequency" sense; for statements of probability are clearly theories. A rejection of his analysis on this point therefore entails a rejection of his solution of the problem of induction in the form which he gives it, for reasons already stated. There are, however, a number of supplementary observations I wish to make. (a) It seems to me that the theory of "inductive verifiability" which Prof. Reichenbach proposes, does little to advance the problem as it was initially formulated other than to restate it in different terminology. For he admits that the problem is insoluble in a two-valued logic, but claims that it can be answered in his many-valued scheme (p. 394). If the latter scheme cannot be translated into the former, his answer is incomparable with the question originally put; while if it can be so translated, a solution must be stateable within the two-valued scheme. Since Prof. Reichenbach raises no difficulties about the admissibility of general statements with any degree of complexity in their quantifying operators, the entire problem seems to me to be reduced to formulating the conditions under which such general statements are accepted. There is, therefore, no special difficulty involved in defining probability as a *limit* of ratios, since probability statements so interpreted are simply a particular type of such general statements. In discussing the methods for estimating and validating the hypotheses concerning the value of a limit, Prof. Reichenbach seems to me to overlook the large amount of "convention" which enters in deciding how much deviation from the hypothetical value will be regarded in a given case as compatible with the hypothesis. While I cannot enter here into an adequate discussion of the matter, I think that an excellent case can be made for the view that statements of probability are not evaluated in terms of higher-order proba-

abilities, as Prof. Reichenbach thinks, but that their acceptance is determined by "conventions" concerning permissible deviations—which are functions of the purposes of the particular inquiry, the sensitivity of the experimental techniques employed, and the resolutions made for carrying on the inquiry within the framework of certain kinds of theories. (b) Prof. Reichenbach claims that the evaluations (in terms of second-order approximate wagers) of first-order wagers are important, because the second-order frequencies converge more rapidly than the first-order ones (p. 405). The mathematical argument at this point is not clear to me, and probably my difficulties with it rest upon some misunderstanding. But in any case, since we are never permitted to know whether we have *reached* the place of convergence in a series, I fail to see that the reason offered for the importance of second-order wagers, even if the mathematical argument is accepted, is of any use in making an estimate of the "goodness" of the primary wager. (c) While Prof. Reichenbach is careful in saying that the Inductive Rule does not describe the (psychologic) process of discovering theories, he does seem to claim that the Rule expresses the method of selecting one theory from a set of proposed alternatives and of progressively "correcting" a theory. "Was von mir behauptet wird, ist, dass die Gewinnung der hypothetischen Sätze durch ein Verfahren erfolgt, in welchem der Wahrscheinlichkeitsbegriff eine Rolle spielt; ich behaupte, dass die bereits vorliegenden Beobachtungen gewisse Sätze wahrscheinlicher machen als andere, und dass die damit gegebene *Rangordnung der Hypothesen* die Entscheidung der Naturwissenschaft bestimmt. So machen etwa die zwischen den Lebewesen verschiedener Rassen bestehenden Ähnlichkeiten zusammen mit den in der Paläontologie festgestellten Beziehungen zwischen Fossilien und Gesteinsarten die Abstammung von einer gemeinsamen Urform wahrscheinlich, so dass man die Darwin'sche Theorie wahrscheinlicher nennen muss als eine Theorie, welche das beständige Nebeneinander aller Rassen seit der Existenz der festen Erde behauptet. Das Verfahren, das wir in diesem Sinne für die Aufstellung der Hypothesen benützen, heisst das Verfahren der *Induktion*" (*Erkenntnis* Bd. V, p. 271). By induction Prof. Reichenbach here means the Inductive Rule. Nevertheless I find it difficult to know just what he does claim for the Rule. It is generally acknowledged by scientists who have reflected upon their methods (e.g., Einstein, in *The World as I see It*), that science is not built up by the process of "induction" or empirical generalization. And I take it for granted that Prof. Reichenbach accepts as a well-established fact that no theory is uniquely determined by experimental findings and that the aim of theoretical science is to find some general principle which will logically unify the diverse empirical material. For if it is a fact, then there is considerably leeway for the operation of traditional and esthetic factors in the logical selection of a scientific theory. Consequently, I cannot believe Prof. Reichenbach really intends to suggest, what seems implied in his views on the role of the

Inductive Rule, that the contrary is the case ; and I suspect that my reading of him must rest upon some misunderstanding of his intent. (Nevertheless, other writings of his support this construction ; e.g., *Handbuch der Physik* (Geiger u. Scheel), Bd. IV, p. 16, 32).

These comments are not intended to cast doubt upon the fact, which Prof. Reichenbach has so well emphasized, that the sciences employ a self-corrective method, and that finality is not to be found in any statement certified on the basis of that method. On the contrary, it seems to me that he has described clearly and persuasively a highly important methodological principle employed in statistical research. I have simply questioned Prof. Reichenbach's detailed account of that method considered as the exclusive and universal rule of procedure in the natural sciences. Moreover, his discussion of induction as a methodological postulate whose use does not "presuppose" *a priori* knowledge of the world seems to me enormously clarifying and in the main correct. Consequently, while I have found it necessary to question some of the important theses in this book, there is no doubt in my mind of its high merit and of the important need it fills in the discussion of probability. It is to be hoped, however, that Prof. Reichenbach will not rest on his laurels, but supply the details in his views which he has thus far simply sketched.

ERNEST NAGEL.

Philosophy and History, Essays presented to Ernst Cassirer. Edited by RAYMOND KLIBANSKY and H. J. PATON. Oxford : at the Clarendon Press (London, H. Milford), 1936. Pp. xii, 355. 25s. net.

THE twenty-one essays which form this volume are the fitting tribute of European scholarship to Dr. Ernst Cassirer on the occasion of his sixtieth birthday. They are focussed on the problem of the relation between philosophy and history, a problem of the first importance in the field of enquiry to which Dr. Cassirer has devoted his life. Half a century has elapsed since Dilthey, in revolt against Kant's exclusive regard for physics as the type of speculative knowledge, appealed to the newly discovered world of the *Geisteswissenschaften* to redress the balance of the old. No living thinker has done more than Dr. Cassirer to bring Dilthey's pregnant, but unsystematic, suggestions to fulfilment. It is encouraging, at a time when a frenzy of nationalism threatens international collaboration even in things of the mind, to find scholars from so many countries uniting to do honour to their colleague. The editors are to be congratulated on the skill with which they have carried out their task. It is not easy, with contributors who represent so many different schools of thought, to secure in the contents of the volume the unity promised by the title-page. Yet the reader

is never allowed for long to lose touch with the main theme, which, as is remarked in the Preface, is one that, in this country at all events, has suffered undue neglect. The range of questions that it opens out is exemplified by the two essays in *Kunstgeschichte*, which witness, it may be noted, to the interest shown by the Warburg Institute in the *Festschrift*. In the first of these, Dr. Saxl, the Director of the Institute, traces the history of the adage *Veritas filia temporis* in the iconography and pictorial art of the seventeenth and the two following centuries; in the second, one of his former associates, Dr. Panofsky of Princeton, does the like for the legend *Et in Arcadia ego*. Judged as original researches in a special field of knowledge, these two papers are the most striking in the collection. But the writers develop their erudition to wider issues; they are at pains to show, in consonance with the purpose of the volume, how the study of iconography involves that of its context, in classical philology, in literature, in the vicissitudes of political and religious history, in the allegorical treatment of speculative ideas; in a word, in the changing temper and outlook of a whole epoch of human civilisation. The art of Nicolas Poussin, for example, gives expression to the Platonic conceptions of truth and of the Good, and, again, to the idea of metamorphosis which, Dr. Panofsky tells us, was "central in Poussin's metaphysical creed". These two studies illustrate how Art, as well as History, takes rank among the "Symbolic Forms", the philosophy of which has been expounded by Dr. Cassirer in the best-known of his published writings.¹

Most of the writers introduce their subject with a reference to the history of modern thought. Of the five contributions from Paris, four (those of MM. Brunschvicg, Gilson, Bréhier and Lévy-Bruhl) are wholly concerned with this line of approach. The establishment by Descartes of the physico-mathematical method as the universal method of reason, and the consequent exclusion of history from rational knowledge; the reactions thereby provoked and the impotence of a philosophy of history as conceived by Hegel and by Comte to bridge the gulf; the development of the biological sciences and the yet more recent revolution in physics through the acceptance of relativity, which have led to recognition not merely of the claims of history side by side with those of science but of a historical factor in all knowledge: these related themes are handled, as might be expected from such eminent thinkers, with an originality and a variety of emphasis that reflect the distinctive interest of each author. M. Brunschvicg, for instance, stresses what

¹ The plates illustrative of both articles are excellent. But is Dr. Saxl right in saying (204), in reference to the illustrations in Marshall's *Goodly Prymer* and in Marcolino's *Cinque Messe*, that "both introduce an enemy of Truth in the form of a winged monster"? Marcolino's "enemy", to judge from Fig. 2, is unwinged. There is a misprint in note 1 on p. 224, where for "p. 23 f. n. 1" should be read "p. 237, n. 1" (? p. 233, n. 1).

M. Bergson has called the "fabulatory function" of history, illustrating from Pascal the exposition "with unequalled clarity" "of the two antagonistic forces—the progress of positive science in the domain of reason which is properly spiritual, the primacy of faith in the domain of religion which is properly supernatural" (29-30), and from Hegel the unnatural union of the legacy of theological fable with the "claim to follow a positive and rational method". "That" in his philosophy of history "which was called history was not history; that in it which was called science was not science" (31). This "fabulatory function", dominant in Marx and Nietzsche, assumes "its clearest self-consciousness" in Georges Sorel, who, in his *Réflexions sur la violence*, "provided a metaphysic for the dictatorships of the extreme left and of the extreme right which have established themselves in post-war Europe".¹ M. Brunschvicg's somewhat cavalier denunciation of Plato as a traitor to truth in his use of fable, and of medieval philosophy as "absurdly subjected to the authority of the ancients", is offset by M. Gilson's careful analysis of the grounds that led to the Cartesian revolution against the medieval synthesis in favour of physico-mathematical science. The scholastics erred in universalising the specifically biological method which they inherited from Aristotle. Hence the sterility of the Middle Ages in the domain of physics, to which that method was inappropriate. [But were they not equally sterile in the field of biology?] Descartes, in turn, was guilty of a like error, when he universalised the method of mathematics, so that "we are bound to condemn the scientific sterility of the Middle Ages for those very reasons which to-day make us condemn the philosophic sterility of 'scientism'" (71). Descartes' error, indeed, cut deeper than that of the medievals; for it was rooted in his definition of existence in terms of thought and his reification of concepts, whereas scholastic conceptualism rested on a thoroughly objective view of substantial reality transcending the sum of concepts abstracted from it. M. Gilson goes on to develop the resulting antinomies, both in metaphysics and in political theory (*e.g.*, the sheer individualism that culminates in Nietzsche and the sheer collectivism that culminates in Comte), and the failure of the attempts, from Kant onwards to Bergson, to justify philosophy "in an order foreign to that of rational knowledge". His own solution lies in the idea of a Christian philosophy, based on the scholastic presupposition of real being, but freed from error by full recognition of the several orders of being and of the diversity of methods appropriate to the science of each order (physics, biology, psychology, etc.). The reader will, we think, be somewhat startled by M. Gilson's rapid identification of metaphysics as the science of being *quâ* being with the theology of Christian Theism. He may wonder, too, what place could be found, in M. Gilson's scheme of knowledge, with its

¹ Cf. Mr. Christopher Dawson's remarks on Sorel in *Religion and the Modern State*.

exclusion of biological concepts from physics, for a theory such as Prof. Whitehead's "philosophy of organism". M. Bréhier naturally surveys the ground from the angle of the History of Philosophy. Two points in his very able paper call for special notice. He sets in clear relief the constructive service that the Cartesian revolution rendered to history. In the relegation of history to *belles-lettres* and in its opposition to traditional authority, the thought of the seventeenth century was assuredly unhistorical; but by its very polemic against the reliability of history it gave birth to the new science of historical criticism. The first-fruits are seen in Spinoza's *Tractatus Theologico-Politicus*, and the direct outcome was the scepticism of Bayle's *Dictionnaire*. Bayle's "historical Pyrrhonism", writes M. Bréhier, "has the same destructive effect on what has been given by tradition and on the evidence for it as that produced by the Cartesian 'doubt' on the constructions of the imagination" (163). The seventeenth and eighteenth centuries were, in fact, an epoch of serious historical research, in which history securely established its claim to be a scientific discipline. In the second place, M. Bréhier analyses acutely the reaction which this Pyrrhonism evoked in the direction of *a priori* theories of intrinsic continuity in the successive stages of human thought, as exhibited in Condorcet, Comte and Hegel. It led in the event to the exclusion from history of "whatever did not humour their philosophical Messianism" (170) and to a closing of the door on historical research. M. Bréhier's own position rests on a rejection of any narrow rationalism and a frank acceptance of the presence of contingency in history: "in history we have to deal not with pure ideas linked together, but with persons who make decisions" (170). M. Lévy-Bruhl's brief paper on *The Cartesian Spirit and History* is confined to expounding their natural antipathy, which, he notes, is still alive, e.g., in M. Paul Valéry's brilliant onslaughts on the pretensions of modern historians to knowledge. Señor Ortega y Gasset, whose essay on *History as a System* is the longest in the volume, employs the historical approach in order to show how in all ages knowledge has rested on an 'operative' faith,—i.e., a collective conviction dominating the thinking of individuals in a given society and epoch,—whether in a religious revelation, or in physico-mathematical reason, or, as he believes for the time now coming, in historical reason. "My purpose", he says (321), "is . . . to discover in history itself its original autochthonous reason. Hence the expression 'historical reason' must be understood in all the rigour of the term; not an extra-historical reason which appears to be fulfilled in history but, literally, a substantive reason constituted by what has happened to man, the revelation of a reality transcending man's theories and which is himself, the self underlying his theories". It is for the *Geisteswissenschaften* to make this rational faith "operative". The place of faith within the scope of reason is indicated also by Dr. Klibansky, in the interesting paper that stands last in the volume (on *The*

Philosophic Character of History), when he shows how in philosophy and science, as well as in history, "the irrational elements of faith have found here", *i.e.*, in their presuppositions which defy verification, "on the very soil of *ratio*, a habitation from which *ratio* can never drive them away" (336).

History, then, has at long last come into its kingdom; it has won recognition as knowledge. But what is its relation, as a form of knowledge, to other forms and, especially, to philosophy? We are here face to face with the main question, which admits of very diverse answers. The phrase "equality of rights" is notoriously ambiguous, in theory as in practice. Prof. Alexander and Signor Gentile, for instance, stand at opposite poles. For the former, all reality and all knowledge are alike historical; Space-Time is the very stuff of which the universe with all that is therein, even God, is made. But, being a realist, he draws a sharp line between the historical reality and the historical knowledge of it: history, he holds, is "but one of the sciences which arise from the facts and happenings of the world" (25). Signor Gentile, on the other hand, roundly denies any distinction between *res gestæ* and *historia rerum gestarum*, maintaining that all history is contemporary history and that history and philosophy are identical in the act of *pensiero pensante* which "embraces all times and the birth and death of all things" in an eternal present (99). For history there is nothing timeless; for philosophy, the time of history belongs to the moment of objectification in the Spirit's dialectic and "only lives by dying into thought and being reborn *sub specie æterni*" (105). We shall not dwell on these two papers, as the doctrines of their distinguished authors are familiar to all readers of *MIND* from *Space Time and Deity* and the *Teoria dello Spirito come atto puro*. Signor Calogero's remarkable contribution *On the so-called Identity of History and Philosophy* calls for fuller notice.¹ He begins by criticising Croce's recently-qualified statement of the doctrine on the grounds (a) that the identity is reduced to a mutual implication which is in fact a circular process of give and take between "the universalizing thought of the methodologist and the individualizing thought of the historian", and, especially, (b) that philosophy and history are each called upon to play a double rôle, since each is at once pure *theória* and the consciousness of *all* forms—including the theoretic—of spiritual experience. Gentile appears to escape these objections by his denial of the plurality of spiritual forms and the consequent reduction of the identity in question to that of philosophy with *its own* history. The 'real' history of philosophy is "the thinking about past philosophies which is actually going on at the moment in the mind"; while, if taken to mean "philosophical development supposed to exist as an independent object preceding and determining our actual present thought about it", it falls within the

¹ Both this paper and Gentile's have been rendered into English by Mr. E. F. Carritt in translations which are masterpieces.

moment of objectification and is an "unreal" abstraction. But Gentile's theory presents insuperable difficulty, in that history is explained away, and philosophy, *i.e.*, the present act of thinking, is left in exclusive occupation of the field. Moreover, the dualism detected in Croce reappears in Gentile in altered guise; behind the philosophy which is asserted to be the single form of the spirit lies a more fundamental philosophy, revealed in the living thought that formulates that assertion. Signor Calogero's argument is hard to compress, but his conclusion is that all epistemology must be self-contradictory, since thought in defining itself must transcend the limits of the definition. "No theory of knowledge is ever universal and necessary, since it has always to admit one exception, namely, the thought it is now thinking. And on the theory that this thought includes all reality, so that the thought it is now thinking is the only one which deserves the name of thought at all, the exception proves to be so considerable as to deprive the rule of any philosophical application" (42). Signor Calogero appears to accept Gentile's teaching as final within the realm of epistemology. "On the level of pure thinking, everything is a mere idea, as Berkeley held, and there is no more to be said." He finds refuge from the impasse in the consciousness of practical activity. The only absolute philosophy is a philosophy of action, which does justice to the claim of the future as the realm of open possibilities. "This difference, between perceived reality and possibilities conceived, forces itself upon us with irresistible self-evidence. . . . To say that the subject is the eternal synthesis of past and future history, that the necessity of consciousness is the common basis of reality and of possibility, that the present is incessantly fashioned by the passage of the future into the past: all these are only different ways of defining the self, that is to say, the conscious will" (49). Our dissent both from Signor Calogero's premisses and from his conclusion in no way diminishes our sense of the value of his paper. His position is as far removed from the crudities of Pragmatism as from those of naive Realism; rather he shows us how Italian Idealism—*quod minime reris*—can, by a skilful turn of speculation, proffer an *eirenicon* (*e.g.*) to Dr. Whitehead's *Process and Reality*.

The last quotation suggests a number of specific questions, relevant to the relation between Philosophy and History, which form the subjects of other contributions. (I) There is, first, the problem of Time. Gentile stands alone in holding that the reality of history involves the denial of its temporal character. It is generally agreed that history implies the reality of time and, particularly, of the past as past. "Its purpose", writes Dr. Huizinga, in discussing its definition, "is to understand the world *in* and *through* the past" (5). A few contributors indeed (*e.g.*, Dr. Medicus, 151) coquet with the view, which most historical scholars will indignantly reject, that the historian's interest lies in the bearings of the past upon the present, rather than in the past for its own sake. Prof. Stebbing,

in an able article on *Some Ambiguities in Discussion concerning Time*, concerns herself mainly with McTaggart's doctrines, on the ground that he "alone makes clear what exactly it is that he is denying when he denies that Time is real". We agree with her, and with McTaggart, that his A-series (of past, present, future) is fundamental, and that, if this be proved self-contradictory, Time is doomed. We agree, again, that "temporal succession involves causal concatenation" and that this view can draw support from recent speculations on Relativity; though we would ourselves go further and maintain, what Prof. Stebbing denies, that the causality implied is that of conscious human agency. But what does she mean by saying that when we call the B-series (*i.e.*, before and after) temporal, 'temporal' is "a descriptive epithet of a curious kind"? "The B-series", she writes, "can be said to be a temporal series because the terms, arranged in the timeless order *earlier—later*, are events When the terms are regarded from the point of view of *earlier—later*, they are regarded as in a serial order which it is convenient to call 'temporal', since the elements ordered are temporal. But the order is not temporal; it is timeless." Of course the temporal series has no position within itself, and therefore is not temporal in the sense of having time relations to other temporals. In this respect it is like any other serial order, such as the numerical series or the musical scale. But these other orders are timeless in a way in which the temporal order is not, in that their being is not bound up with time-differences as it is with number—or tone—differences. This cannot be said of the temporal order. The order of earlier and later events is timeless only in the sense of not being an event in time, not in the sense of being what it is whether there are events or not. Moreover, to say that earlier and later events are temporal, while their order as earlier and later is not, seems to ignore the peculiarity which distinguishes temporal (and spatial) order from all others. We apprehend other orders through first apprehending the different determinate forms which some generic character takes in different particulars; *e.g.*, the lighter and darker shades of blue, the higher and lower tones of musical notes. The terms cannot be resolved into their relations within the order which they form; but their order is grounded in those very differences which resist such resolution. With the temporal order it is otherwise. If the terms are instants, this is obvious: there are no differences between the terms except what the relations constitute. If the terms are events, as Prof. Stebbing holds, we are no better off. For, though events differ one from another in characters which we cannot resolve into their order as earlier and later, these characters do not enable us to order them in time. It is only as having position in the time-series that anything is an event. We come back therefore to the same conclusion. In the order of earlier and later, the terms get their being as terms of the order through the relations which constitute the order. They and

their order are so bound up together that the only sense in which they and not the order can be termed temporal is that the order is not one of them. And that is no reason for calling it timeless and not temporal. We may add that if Prof. Stebbing's argument is valid, it holds not only for the B-series but for the A-series. That too can only be called temporal in the same 'curious' sense. Nor can we allow that it is "nonsense" to say that the order of the integers does not change, in the strict meaning of change as change in time, or, again, to use the expression "true at all times", on the ground that the order of the integers and truth are timeless. Prof. Stebbing seems to us to draw far too rigid a line of demarcation between the timeless and the temporal, making it difficult to bridge the chasm between the two worlds, and to explain either the ingreience of "eternal objects" in temporal "occasions" or the apprehension by beings in time of those timeless objects (*cf.* Plato's *Parmenides*).¹

(II) There is also the problem of the relation of history to religion. Signor Gentile rules God out of the picture: charging "the theistic philosopher, whether Platonist or Aristotelian, who presupposes God", with excluding God from history (92). For Prof. Alexander, on the other hand, God is on the move; He is through and through historical. Dr. Clement Webb, writing all too briefly on *Religion, Philosophy and History*, after showing how History and Philosophy alike had their birth in Religion, and severed themselves, in the course of man's development, from the parent stock, vindicates at once their autonomy in face of religion and their close relationship with it. He criticises Croce's view of religion as immature philosophy on the ground that Croce sees in religion nothing but a cognitive activity, whereas its function is to place man in contact "with what he divines to be at once at the back of all that he experiences and also of his own experiencing self" (55). The beliefs which express religious experience are not sacrosanct from philosophical or from historical criticism—far from it; "religion cannot be satisfied with a false world-view or with a sense of continuity with a past that was never present" (59). But no philosophy or history can hold its ground which can be shown to be inconsistent with the fact of religious experience; and Dr. Webb insists at the close of his paper that the consciousness of selfhood revealed in the recognition of "that all-embracing and all-permeating reality, at once 'transcendent' and 'immanent', which we divine to lie behind and within all which enters into our experience" must be frankly acknowledged by philosophy as a fundamental that withstands criticism (60). Its rejection would leave philosophy with an unsolved enigma on its hands. Perhaps the most valuable point in this paper is the insistence on the personal and social character of man's intercourse with God in worship, which finds expression in the use of the second personal pronoun

¹ In these criticisms I owe much to discussion of Prof. Stebbing's paper with Mr. H. W. B. Joseph.

to replace the third. A similar point is made by Dr. Groethuysen, in reference to man's knowledge, not of God but of himself. As thought works on the datum of naive self-acquaintance, "the original monologue of self-reflection becomes a dialogue", the personal "I" being de-individualised as "he", till, at a yet higher stage of self-knowledge, personality is restored through art and religion, wherein the subject recognises himself as "thou" (77-82). Dr. Groethuysen's philosophy is avowedly anthropological, and the deity of his religion is the self. Man "seeks his soul, his God, the 'thou' with whom his soul holds converse" (82). For Dr. Groethuysen, as for Croce, the consummation lies in a philosophy which at once criticises and synthesises the other forms of self-reflection, art, religion and philosophy itself. In this he is at one with the great majority of contributors, excepting Dr. Webb, M. Gilson and Dr. Hoffmann (who contributes a scholarly paper on Platonism in *St. Augustine's Philosophy of History*); the rest, if they quit the fence that parts a theistic from a non-theistic metaphysic, descend upon the side of humanism. This resolute determination to accept any metaphysical solution, rather than the theistic, of the speculative—and, we may add, the practical—difficulties in which the world has been embroiled, ever since its humanistic aspirations swung free from their original religious moorings, strikes us as somewhat paradoxical. When Dr. Medicus, for example, tells us that "history is governed by the destined, though of course not assured progress of mankind towards humanity" (155)—if destined, how can it fail to be assured?—and that "the idea of humanity is the transcendental principle of all historical knowledges. It is through it alone that historical knowledge acquires unity" (151), what precisely does he mean? If he means merely that the historian judges the significance of historical persons and events by a relative and changing standard of human civilisation, ruling out of account any explanation by reference to supernatural intrusion as beyond the pale of his enquiry, these statements are as true as they are trivial. But if he means more than this, and claims that the historical process finds its sole and adequate explanation as a theodicy of Man universal, towards whose historical consummation all creation is groaning and travailing together in pain until now, such a doctrine is both barren and erroneous. It is but a republication, in a new metaphysical dress, of the discredited Comtian apotheosis of humanity. Is it not time that the prophets of humanism should take to heart Bradley's strictures on the emptiness of the concept of humanity, save when integrated with religious faith in an other-worldly order, and M. Bergson's contention, recently developed in *Les deux sources*, that the love of man for man, beyond the bounds of an actual 'closed' society, is only possible when it is rooted in the love of God?

(III) Dr. Medicus' paper *On the Objectivity of Historical Knowledge* is not the only one that treats of this problem. Dr. Huitzinga,

for instance, while admitting "the unavoidable subjectivity implied in every history", sees herein no ground for scepticism, any more than in philosophy or science. "Every civilization", he tells us, "has a past of its own", though he adds, more questionably, that "our civilization is the first to have for its past the past of the world, our history is the first to be world-history" (8). As Dr. Webb truly observes, "the sense of continuity with the particular past of a community to which oneself belongs" (58) was the primitive impulse to historical enquiry, and still frequently persists as a motive in the minds of the most disinterested historians. On the part played by the historian's mind in knowledge, Dr. Huitzinga is a little ambiguous. He holds rightly that the historian's interest is "to penetrate to the genuine knowledge of that which truly happened" (6), and that the purpose of history is "to understand the world in and through the past" (5). Yet he speaks—loosely, we fear—of the historian's "imposition of form upon the past" (5), and tells us that "every civilization"—i.e., the 'thinking subject' concerned (oh! these abstractions!)—"creates its own form of history" (7). Dr. Medicus stresses the personal factor in history in sharp contrast to the impersonality of physical science. Historical knowledge, he says, rests "on the basis of the *personal history* of the men who are striving after it" (154). The historian's construction is coloured by his *Erlebnisse*. His vision, as Goethe said of himself after studying Herder, is of "eine gefühlte Welt". "Knowledge of the mathematical sciences of nature", on the other hand, "is separable from the individual who knows it: as objective knowledge it claims to be valid for consciousness in general (*Bewusstsein überhaupt*) and so for every subject in the same way" (*ib.*). This appears to us to be very precarious doctrine. Of course, the historian's work is conditioned by his limitations, so that his history is a perspective of the past, seen from the angle of the age in which he lives; but does this preclude impersonality, in the sense that he cannot make an impartial judgment? "It belongs in no way to the service of truth and humanity" (what has 'humanity' to do with it?) "that the historical presentation should be colourless and neutral, that it should be impartial" (149). An impartial presentation may be anything but colourless: it is only false colour, as in the political propaganda justly criticised by Dr. Medicus himself (145, 148), that should be excluded from history, as from science. As Dr. Wind points out, in his essay *On some Points of Contact between History and Natural Science*, in both fields of enquiry the investigator is part of the world he investigates; the difficulty of eliminating the personal factor is doubtless harder for the historian than for the physicist, but it is there for both.¹ Dr. Medicus'

¹ In Dr. Wind's contribution we note two minor errors. It is incorrect to speak (256) of the "principate" of Pompey, who was merely *princeps senatus*. And "Copernican emotion" (260) is a misprint for "Copernican revolution".

counsel, that we should seek the desired objectivity of historical knowledge in a 'supra-historical standard' of 'pure humanity' (156-157), is neither fruitful nor convincing. Dr. Klibansky is surely more illuminating on this question when he argues that the objections raised against history on the score of its subjectivity lose their force when it is recognised that the notion of objectivity is not "a *fixum* confronting the thinking consciousness" but "a pure intention of thought itself". It is "an idea . . . in a three-fold signification; in the Stoic sense of a germ of reason, a force stirring consciousness; in the Kantian sense of a regulative principle ordering phenomena; in the Platonic sense of a normative pattern towards which the empirical form of human knowledge is directed" (336).

IV. The real difference between history and the sciences of nature surely lies in this, that the interest of the physical scientist is in the establishment of universal connections, while that of the historian is in tracing unique and unrepeatable patterns among unique and unrepeatable events. Here, as everywhere in philosophy, we are confronted with the secular problem of universals. Dr. Litt, alone among the contributors to this volume, sets himself to consider this problem. We cannot enter, at the close of a long review, upon the full discussion that his very able essay, *On the Universal in the Structure of Historical Knowledge*, deserves. The question, like so many others in this field, was raised, but not answered adequately, by Dilthey, who ignored the rôle of universal meanings of words in history; Rickert remedied the omission, but interpreted these first linguistic universals as though they were impersonal class-concepts. They are in fact anything but impersonal, being pervaded, as Dr. Litt shows, following Dr. Cassirer, with individuality of meaning. "The task of historical thinking is to grasp and hold fast a unique form in its whole concreteness and vivid determinateness. . . . This purpose will be realized, both in the case of the speaker and in that of him who understands what is spoken, exactly as, and only in so far as, those expressions are used and understood as indicating concrete fullness of content, without any thought of arranging things under classificatory abstractions" (131). But, over and above these two kinds of universals, Dr. Litt points to a third kind, of the highest significance for history and the *Geisteswissenschaften* generally, "which, though not in the least inferior in strictness and certainty of inner grounding to the abstractions of classifying thought, by no means achieve their content by revealing what is 'common' in a multiplicity of phenomena: they rather bring to expression a universality, resting on itself and providing its own ground, that can be grasped, if we look in the right place, for and in every 'particular' of the spiritual world" (135). Dr. Litt's remarks on these 'concrete universals', or, in his own more fitting phrase, "*a priori* fundamental concepts", are all too brief and leave us asking many questions. What, for instance, is their rela-

tion to the unique patterns, such as, *e.g.*, the Reformation in Germany as conceived and interpreted by von Ranke, which, though assuredly not *a priori* are individual concepts, realised, if not throughout the world of Spirit, "for and in the particulars" which compose them? Dr. Litt's remarks on the meanings of words connect his paper with that by Dr. Pos of Amsterdam, in a later part of the book, on *The Philosophical Significance of Comparative Semantics*. The present reviewer regrets that he is not qualified to do justice to the interesting views put forward in this article, or to many other of the varied topics raised throughout the volume. What has here been written by way of appreciation and of criticism will suffice at all events to show the value of the *Festschrift* for contemporary thought, alike by reason of the intrinsic merits of its contents and its opening out fresh avenues of thought on problems of high importance both to philosophers and to historians.¹

W. G. DE BURGH.

The Rise of European Liberalism. By H. J. LASKI. London: George Allen & Unwin, Ltd. 1936. Pp. 287. 7s. 6d.

THIS book is an attempt to reinforce the argument of the author's previous work, *The State in Theory and Practice*, by a historical account. It sets out to describe the development of liberalism in Europe, beginning with the Reformation and going down through the seventeenth and eighteenth centuries. These three periods are fully discussed in the three main chapters of the book. We should then expect a still fuller discussion of the nineteenth century, which Prof. Laski describes as "the epoch of liberal triumph", and which certainly saw the most complete and significant developments of the liberal point of view. But, for some reason, this period is dismissed in a few perfunctory pages in the "Conclusion", which is mainly concerned with the beginning of the downfall of Liberalism in the post-war period: even that is treated comparatively briefly in what is little more than a summary of the argument of *The State in Theory and Practice*. It is difficult to see any reason for this neglect of the most important period, and it is certainly a grave defect in the book.

Judging it by what it does do, one may say that the book shows many of the characteristic excellences of the author's work, great learning, deep earnestness of purpose, and an attractive and readable style. Some of the incidental discussions of particular points are

¹ A complete bibliography of Dr. Cassirer's published writings, classified according to contents, by Dr. Klibansky and Dr. Solmitz, is appended to the contributions. The index is excellent. Praise is also due to the various translators for their English renderings of the articles by foreign scholars.

very convincing. For instance, the view taken of the relations between Protestantism and Capitalism seems to me much sounder than that associated with Weber and his followers. But the book as a whole is disappointing. What is perhaps the fundamental defect is that Prof. Laski never seems quite clear, or at any rate never makes it clear to others, what it is exactly that he is discussing.

It is possible to take a point of view or a doctrine, or a set of ideas or aspirations with some sort of inner connection, and to trace its emergence and development. One could show what influences helped its adoption in some quarters or at some times and hindered it in others, how far it succeeded or failed, and what results it produced other than those at which it aimed. One would expect to find that certain groups at certain times adopted these ideas very partially and imperfectly; for instance, they might limit their application to their own circle, though the ideas themselves called for a much wider application. There would, however, be no justification for including these inconsistencies and limitations as part of the ideas themselves. For instance, the idea of toleration of opinion has often been put forward by groups who in practice were really only interested in its application to themselves. But it would be absurd to describe this limitation as itself part of the idea of toleration.

On the other hand, it would be possible to take a particular concrete group of people, and to show how its special situation and special interests might incline it at times to one set of ideas and at times to another, or to certain aspects of a particular doctrine and not to others. My criticism of Prof. Laski is that he wavers between these two lines of approach, or rather, perhaps, that he claims to be following the first whereas most of the time he is really following the second. A great part of his space is occupied, not with the development of liberal ideas, but with the development of the views of the business classes in so far as these were determined by their desire for wealth. He has no difficulty in showing how, in so far as they were under the influence of this motive, they sometimes supported authoritarian government and sometimes resisted it, or put forward claims for individual liberty and rights which they had no thought of applying beyond themselves.

It must be noted that this line of approach is just as much an abstraction as the other. The business classes were not moved solely by the desire for wealth, nor were they the sole influence which controlled the development of the period. Prof. Laski warns us from time to time against over-simplification or taking abstractions for concrete realities. But he continually forgets his own warning and tends to speak of a particular class as an absolutely uniform body with a coherent and consistent policy directed to one end. What is worse he is inclined at times to personify the varied and often inconsistent ideas which influenced them as an

individual policy or philosophy and to speak of "its" aims and desires.

His central thesis seems to me to rest on a confusion of this kind. Liberalism, he says, has in fact taken the form of resistance to arbitrary authority, of the advocacy of toleration of opinion, of the demand for individual liberty—"the right of the individual personality to make its own terms with life"—, of a belief in reason. But these are only "the ends its [whose?] more ultimate purposes caused it to serve". These "more ultimate purposes" appear to be the claim to pursue wealth as an end in itself without any restriction—"the pursuit of wealth for its own sake becomes the chief motive of human activity"—, and the assertion of the rights of property not only against authority, but also against the non-propertied classes. Consequently the liberty and the rights claimed are only claimed for men of property.

There is plenty of truth in this as a historical account, though it is a one-sided truth. But to speak of it as Liberalism shows a bad confusion of thought. It would be obviously absurd to suggest that there was anything in the idea itself of individual liberty or, to use Prof. Laski's phrase, "the conviction of the innate dignity of human personality", which necessarily confined it to the owners of property, or associated it with the pursuit of wealth as the sole end. The interesting line to follow is to examine how the liberal idea or ideal was encouraged up to a point by these other motives and influences and then checked and limited by them, inconsistently with its own inherent character. The process should be presented as an interplay of different and sometimes opposed forces, acting and reacting on each other. But to do this the different forces must be distinguished. To group them all together under a single heading marks a failure in analysis.

To complete the picture we ought to be shown the other side of the process. We need to see how the liberal idea pressed against these limitations and in its turn gradually imposed very real limitations on the rights of property and the unrestricted pursuit of wealth. That would be part of the history of the nineteenth century, which we miss in this book. But we could hardly expect it from Prof. Laski, for to him no restrictions on the rights of property are of much significance if any rights over property are left at all, and the failure to accept the complete Marxist analysis is very little different from denying any rights to non-propertied classes at all.

This refusal to allow any independent force to influences other than economic, weakens his power of analysis and his historical imagination alike. The fact that a certain tendency or set of ideas is favourable in some degree to the purely economic aspirations of business men is not, as the argument seems to regard it, a proof that it is caused by them alone or even mainly, nor does the fact that certain tendencies show themselves in economic activities, as well as elsewhere, prove that the economic expression is any more real or fundamental than the others.

Thus, Prof. Laski is no doubt right in regarding the impatience felt by the enterprising business man at the restrictions imposed on his activities by antiquated customs and institutions as very much the same kind of feeling as the impatience felt by the scientific researcher at the limitations imposed on his activities by non-scientific authority. But this is no proof that such a feeling in science is a secondary product of the feeling that originates in the desire for the acquisition of wealth. The feeling is something much more general than any one expression of it. But the whole question of the development of the scientific attitude provides a very good instance of the inadequacy of Prof. Laski's analysis. He asserts that "the new outlook codified in the *Principia* of Newton emerged from a nexus of problems presented to science by business men". He refers, in support of this, to Hessen's article published in *Science at the Cross Roads*. But Hessen's article, interesting though it is, is a very one-sided collection of material, which proves no more than that economic considerations had some influence on the development of Newton's thought, and ignores the large amount of evidence for the other directions from which these problems arose, including what Prof. Laski rather superficially dismisses as "pointless curiosity".

Another instance of one-sided analysis is his account of the growth of religious toleration. "The history of toleration shows", he says, "that it is the economic destruction wrought by civil war which creates the mental climate favourable to toleration". Or, more succinctly, "Tolerance came because intolerance interfered with the access to wealth". But this forgets that intolerance only interfered with the access to wealth because a considerable number of people cared sufficiently for their religion to fight for it rather than to give way. So the religious motive has to be recognised as an independent force if we are to understand the situation that arose at all. Farther, dislike of anarchy and violence is a much wider thing than merely the dislike of interference with the pursuit of wealth, though no doubt it includes that. "The poorest he" in France suffered from the religious wars as much as "the richest he". Or again the desire for a strong national state which played such an important part in the growth of the idea of toleration depended on many motives besides the desire of the trading classes for access to wealth.

Another great weakness in Prof. Laski's presentation is his failure to make due allowance for the force of habit and the carry-over of habits of thought from an earlier age. This comes out particularly in his attitude to the Middle Ages. He certainly idealises the Middle Ages in some of his statements. But what is more important is his failure to see how many of the ideas which he associates with the new age were really survivals from the mediæval period. In particular, the insistence on the rights of property against arbitrary interference by the monarch is thoroughly mediæval, as

could be illustrated by many passages in mediæval writers. So is the connection of property with political power. The landless man was not an object of much consideration to the mediæval mind. And if we read the whole of the famous Ireton-Rainborow debate, instead of confining ourselves to the passages commonly quoted, we can see that the point of view represented by Ireton is fundamentally mediæval.

Audacious though such a criticism may seem from one who has no claim to a tithe of Prof. Laski's erudition, I should venture to assert that he is really lacking in historical imagination. He is too constantly occupied with the thought of the use he can make of his historical material to enforce his view of the situation at the present day. I would admit, or assert, that the ultimate justification of the study of history is to be found in the light it throws on our own problems. But, as it seems to me, to obtain that help we have to forget for a time to think of it merely from that point of view and to sink ourselves in the life and thought of the period we are studying. We have to feel the importance of what the people of the period thought important, not merely of what we should think important at the present day, and to judge what happened in the light of the alternatives that were possible at the time. If we cannot do that, we shall be bad historians, and, what is more important, we shall fail to get the help we could get from the study of history for the understanding of our own situation. It is, I feel, a lack of historical perspective in Prof. Laski which makes him so positive that the conditions of the moment are a certain indication of the developments of the future, and that what may be only a temporary set-back to the spirit of Liberalism is a proof of its final destruction.

G. C. FIELD.

VI.—NEW BOOKS.

Possibility: University of California Publications in Philosophy, Volume 17, 1934, pp. 1-224 (Published in U.S.A. by University of California Press, \$2.25, and in Great Britain and Ireland by Cambridge University Press, 10s.).

THIS volume—an addition to a now celebrated series—consists of nine lectures delivered before the Philosophical Union, at the University of California, in 1933. It includes scattered wisdom on a great variety of subjects: there is hardly a current question that is not touched upon by one or other of the contributors and most readers will find here something which they particularly value. The collection may also serve to emphasise the fundamental unity of problems which are commonly regarded as distinct: the nature of *universals*, for example, is here discussed together with the notion of *Law* or *World-Order*, and the two problems are seen to be one. And, from a more removed standpoint, this book will have an interest for the historian of philosophy, who will take these papers from Berkeley as a fair sample of the intimate views and interests of American academic philosophy in 1933. He will notice, for instance, that the authors are not much interested in High Metaphysics, and yet that most of them seem to assume that even critical philosophy must rest upon a metaphysical foundation; he will notice that all the authors profess an empiricist approach to their problems, and that most adopt some form of modified realism. And again, the historian will find here evidence of the growth of a characteristic American style and tradition—the persistent influence of James and Dewey, and the growing importance of C. S. Peirce, G. H. Mead and Professor Lewis of Harvard.

This task must be left to the future: it is possible here only to outline the main topics debated and to add an enumeration of the many historical and critical discussions to which the main problems give rise.

(a) The problem of Possibility is forced upon us by the contrasted natures of universals and particulars. The concrete particular seems to exclude all alternatives: but the universal is *determinable*. Prof. Pepper says: "A possibility is a specifiable that admits of alternatives; an actuality is a specifiable that does not admit of alternatives" (180). Again, the universal is the *repeatable*: "Thus, to say that self-identical qualities and relations or essences constitute the possible, is to say that they are what can happen again and again in existence; or, in a word, that essences are repeatable" (Dr. Church, 50). But what are "these unities"? Dr. Church seems almost to assert that they are subsistents: but most of the writers are anxious to avoid any such simple-minded realism. "I want objective and constitutive possibilities", says Prof. Adams, "and I do not want subsistent dualism" (9); while—from a very different standpoint—Professor Lenzen makes the same objection: "The realist

... transforms the possibility into a substance when he assumes the reality of the universal which appears in all its instances. ... The possibility of similarity is transformed into a universal which has actuality in a realm of subsistence" (68-69). Prof. Lenzen is inclined to adopt the extreme nominalist position which F. P. Ramsey tried (and failed) to make plausible: "To say that a particular quality of the patch A is an instance of a universal, means that it is possible to find patches B, C, D, etc., such that A is similar to B, A is similar to C, etc., in a specific respect" (64). But this can hardly be regarded as adequate, for the problem seems to recur in his last four words.

(b) The particular also, which is actual, involves the possible. "We do not know the actual," says Prof. Pepper, "except as an extension from our knowledge of possibilities" (181). Mill's definition of matter as "a Permanent Possibility of Sensation" is discussed by Professor Loewenberg (91) and by Prof. Mackay (142 *et seq.*): and Prof. Lenzen puts forward a chastened form of Russell's *Sensum* Theory, according to which "The desk consists of the totality of its given and possible aspects" (61).

(c) But the Possible must be distinguished from the Impossible, as well as from the Actual. Prof. Adams insists that this distinction cannot depend upon any fact about the relation of knower to object. "We discover what is really possible; its existence does not wait upon our knowledge. The framework of the possible is set for us and not by us" (19). Thus, although we must call the unobserved aspects of a body *merely possible* in some sense of "possible", yet this sense of "possible" gives rise to no definite "impossible" and its distinction from "actual" is (he says) purely epistemic. But surely the distinction between *given* and *merely possible* aspects of a body does depend upon perfectly objective facts—facts about the object and the percipient's body.

The distinction of Possible from Impossible raises the questions: What do we mean by Law (logical or natural) and what right have we to believe that such laws are binding in experience? For the possible must be relative to a given system of law, a "context". Various attempts are made to enumerate such systems: Prof. Loewenberg suggests logical, empirical, physical, psychological, systems (101), but rejects the Absolute and the Specious Present. Prof. Mackay names the realms of discourse, of physics, of life, of society, of the imagination (148). Do these systems completely determine what is actual? Are there, after all, no mere possibilities that are not impossibilities? Dr. Marhenke tries to distinguish between causal laws and observed constants—between the general principles and the determinate "Initial Conditions" from which the principles actually operate at any given time (170 *et seq.*). But since the laws are only general facts, and the constants, more detailed facts, about the experienced world, this distinction would seem to be purely epistemic, and not to touch the ontological problem of determinism.

(d) The Possible is, in a certain sense, confined to the future: this is the view of Positivism (Prof. Lenzen 59) and of Contextualists such as Mead. But Prof. Pepper rejects this view in part. The past "cannot be actual. We must take that as settled. But that fact does not exclude the possibility of the past" (190). He holds that the past is real in the present, just as the future is; "On contextualistic grounds there is definite evidence for the possibility of the past" (191). But this leads to another difficulty, for he concludes: "the status of the past and that of the future are exactly symmetrical" (193). And surely we *must* avoid saying this about past and future!

(e) Possibility and Impossibility may perhaps relate to *meaning*. Are round-squares possible? Prof. Marhenke has a complicated argument to show that this cannot be a significant question unless the sign "round-squares" is itself significant. Since it is clear that, in some sense of the word "significant", this sign is *not* significant, Prof. Marhenke rightly discusses various senses of the word "significant". He discovers, in fact, a hierarchy of some five or six senses of the word, and argues that if S_n be a criterion of *Possibility*, the question: "Is A possible in the sense S_n ?" is significant only if the sign "A" is significant in the sense S_{n-1} (162). It seems to me that this is unnecessarily elaborate. "Are round-squares possible?" may surely signify the same as "Is 'round-squares' significant?" And this latter sign means a sensible question if only "round-squares" is a *sign*—whether it be a significant sign, or a non-significant sign.

Amongst the other topics discussed in this book are the following: *Pluralism* by Prof. Savery ("Concatenism") and Prof. Loewenberg; *Positivism*, in a clear exposition by Prof. Lenzen; *Contextualism* by Prof. Adams and Prof. Pepper; the nature of *Mathematics* by Prof. Lenzen (60); *Time*—McTaggart's "B-Series"—by Prof. Pepper (193); *Space and Measurement*, by Prof. Lenzen (73 *et seq.*); *Strict and Material Implications* by Prof. Mackay (134 *et seq.*). Besides these, the student will find valuable comments on famous philosophies; *Descartes and Spinoza*, by Dr. Strong (122, etc.); *Leibniz*, by Prof. Loewenberg (87-90); *Hume*, by Prof. Adams (20) and Prof. Mackay (43); *Mill* by Prof. Mackay (142-144) and Prof. Loewenberg (91); *Bradley* by Prof. Adams (22) and, at some length, by Dr. Church (37 *et seq.*); *Bergson* by Prof. Adams (20).

This volume suggests the advantages which a philosopher (like any other literary man) derives from identifying himself with a certain school: for those papers which are framed within the limits, and expressed in the vocabulary, of such a school, seem to me to be the only ones which are intelligible as *wholes*. The other papers offer us those cherished crotchets and isolated intuitions which serve to exhibit the author rather than to illuminate the subject. And this volume will all too readily supply the collector with examples of tautology and nonsense, clothed in oracular or academic style:

"The immediate future is a thick, highly conditional possibility" (189).

"It is barely possible that the actual does not exist" (182).

"We thus neutralize the acidity of the mechanical system with the alkalinity of the vitalistic. The vitalistic cure would not be needed if the mechanical system had not been swallowed without reservations" (112).

"Mathematics is a kind of knitting" (209).

"The language of philosophy, shorn of hoary notions, is in danger of becoming jejune. Besides, the habit of paring away entities seemingly otiose, creates the illusion that nature is as clean-shaven as the intellectual system based upon the principles of parsimony. Nevertheless, the endeavour to avoid reifying possibility is worth making" (183).

KARL BRITTON.

Truth and Corrigibility. An Inaugural Lecture Delivered before the University of Oxford. By H. H. PRICE. Oxford: Clarendon Press, 1936. Pp. 31. 2s.

Eppur si muove! should surely be the first exclamation to rise to the lips of the reader of this remarkable Inaugural Lecture of Oxford's new professor of Logic. For it reveals a radical shifting of the philosophic scenery and climate. No longer are the dreamy spires of that ancient seat of 'learning' set in the stagnant backwaters, and beset by the stale mysteries, of Isis, but they glisten in floods springing from the distant waters of the Danube and the Cam. Gone are the old traditions, or reduced to vestiges that merely testify to their defunctness. Gone is the Aristotelian syllogistic with its claims to formal validity and demonstrative cogency. Almost gone is the old sophistry of verbal dialectics. Gone too is the absorption of logic by metaphysics in the all-engulfing maw of an imaginary Absolute. In short the old Formal Logic seems to have fallen into an utter slump, and the Oxford logicians seem to be going over with bag and baggage into the camp of the symbolic logicians and logical positivists, and surrendering to the claims of empirical science.

Nevertheless closer inspection shows that this process has not yet reached its logical termination, even in Prof. Price's thought. In the first place there are survivors in it of the old Greek logic, such as Plato's distinction between 'Knowledge' and 'opinion', which is admitted to be unworkable, while "there is a *prima facie* case for the view that the distinction is only one of degree not of kind" (p. 4), and Epimenides the Cretan, who is dealt with after the manner of Lord Russell (p. 5). Secondly, Prof. Price is not quite ready to admit that *all* truths are corrigible, though "we must in any case admit that the vast majority of the statements we make in everyday life, in the Natural Sciences, and in History express not knowledge but belief" (p. 30) and so are corrigible. He will admit that "all empirical thinking is corrigible" (p. 31), but tries to restrict corrigibility to "first-order statements" and to hold that "some mathematical thinking" (*ibid.*) is not corrigible. The same is true of the Laws of Logic, because "in the very act of correcting other judgments one presupposes the incorrigibility of the Laws of Logic" (p. 6). But there is no inconsistency, Prof. Price thinks, in the division of the field of logic thus proposed (p. 31).

Unfortunately, it does not seem that this contention is sound. Prof. Price's argument, though it is of an old and familiar type and has often been used to allege, *e.g.*, the impossibility of scepticism, really rests on taking 'corrigibility' in complete abstraction from the actual process of correcting errors. It assumes that the principle of corrigibility must be proved *a priori* and absolutely, apart from its applications, and cannot be held tentatively and justified by its use. Nor does he consider how 'truths' arise in the concrete case. If he did, he would find that a 'truth' was always the accepted (because at the time the most acceptable) solution of a concrete problem, which was arrived at empirically and in a context that provided its premisses. Hence a universal formula is essentially inductive in character.¹ 'All truths are corrigible' means "all the truths whose claims have been examined have been found to arise from the correction of earlier 'truths' which they have superseded". This

¹ Even where it is an outright postulate, its verification is inductive and empirical.

process is unending, and does not involve any claim to infallible prediction of the future. It merely indicates that if and when doubts arise in future about any truth now acknowledged, the same procedures as were used to establish it may be used to correct it. In other words Prof. Price does not always construe 'corrigible' exactly enough. At the bottom of page 4 he explained that a corrigible judgment was one which "may turn out to be false". So a corrigible truth is, not one which *has* to be corrected, but one which *may* be. And the burden of proof lies upon the challenger of its truth.

As for the alleged superiority and incorrigibility of the Laws of Logic and the truths of Pure Mathematics, what has the history of Logic been but the history of the continuous corrections which every later logician has introduced into the doctrines of his predecessors? At present logicians find themselves quite unable to agree what, if anything, the 'Laws of Logic' are to mean. Mathematics has been a much more progressive science than logic. It has progressed through analogical extensions of simple processes of counting and measuring. But of course analogy cannot be regarded as incorrigible, or even valid, reasoning. Moreover, here too, there is no present agreement among mathematicians about the meaning of mathematical first principles, while the philosopher may further protest against attempts to explain pure mathematics in abstraction from applied.

Thirdly, Prof. Price is disposed to think that judgments of perception, introspection, and memory may never need correction (p. 25). But surely it is vain to claim any special trustworthiness for these, so long as perception is inextricably bound up with illusion and hallucination, and introspection and memory are notoriously fallible. Whenever, therefore, any claim is based on a 'truth' of memory, introspection, or perception, it will first have to be established that the process genuinely is what it professes to be. And does not that mean that it is in principle subject to error and correction?

Fourthly, Prof. Price follows the Oxford tradition to the extent of calling the logical units 'judgments' and not 'propositions'. But also, alas, to the extent of calling 'judgments' what are at most 'propositions', and occasionally using the two terms interchangeably (pp. 17, 27, 28, 29). And when it is added that what the bulk of logicians are wont to call 'propositions' are really '*propositional functions*' having only *potential* meaning, while their *actual* meaning varies with their context and their use, it is clear that even 'modern' logic still has some way to go before it can emerge from the morass of Formalism, and give an adequate account of human reasoning.

F. C. S. SCHILLER.

Geometry of Time and Space. By ALFRED A. ROBB. Cambridge: The University Press. 1936. Pp. vii + 408. 21s.

IN spite of a slight change of title, this book is essentially a new edition of Dr. Robb's *A Theory of Time and Space*, which was published in 1914. There is a new and amplified introduction, some of the more complicated proofs have been shortened, and the introduction of new matter has made the treatment more self-contained. But since the theory remains almost exactly in its original form, the intending reader may be glad to be reminded

of Prof. Broad's 'critical notice' of the first edition which appeared in this *Journal* (vol. xxiv., pp. 555-561).

Dr. Robb's book was the first attempt to develop in full detail a formal geometry of spatio-temporal relations satisfying the needs of the special theory of relativity. With the recognition that spatial relations are affected by the velocity of the observer, a geometry limited to spatial relations can no longer appear as more than a crudely imperfect description of the physical world. Minkowski's demonstration that the formulæ of cartesian geometry could be extended so as to be adequate to spatio-temporal and not merely spatial relations, though of fundamental importance, remained imperfect in the absence of systematic exploration of the modifications in the properties of length, normality, etc., which it demanded. In setting out to supply this need, Dr. Robb followed the classical method of Euclid in obtaining every theorem by strict deduction from explicitly formulated axioms. By exhibiting the consequences of the axioms in this way, it became possible for the first time to realise the full implications of the physical theory of which they are the formal expression.

Dr. Robb's choice of the fundamental notions in terms of which all other notions of the system are to be defined is of great interest. Its most striking feature is the absence of any notion of the simultaneity of events which occur at different places. It is well known that the assumption of the constancy of the velocity of light, in accordance with the results of the Michelson-Morley experiment, entails that the simultaneity of events which occur in different places is relative to the velocity of the observer. Dr. Robb rejects this as a paradoxical consequence. While his arguments on this point, in the introduction, are too summary to produce more than a conviction that his arguments must be stronger than they are made to appear, it is of great importance to discover whether alternative methods of formulating the 'paradoxical' situation are possible. Dr. Robb is able to dispense with simultaneity by basing his geometry on the notion of a relation of 'before' between instants whether at the same or different places. The interpretation of this notion (as distinct from its formal properties, which are exhaustively contained in the axioms of the system) is roughly as follows. If any signal can be sent at the instant a to arrive at the instant b , then a is said to be before b (and b is said to be after a). Thus the relation is clearly asymmetric, *i.e.*, if it holds between a and b it cannot hold between b and a . Suppose now that a person on Sirius flashes a light signal to me at an instant t_1 (registered on *his* watch) which I receive at an instant t_2 (registered on *my* watch). Then the instant at which I receive the signal is *after* the instant at which it was despatched. If I immediately reflect the signal back at the instant at which I receive it, so that it returns to Sirius at the instant t_3 (measured on a clock at the place when it is received, again) then the instant t_3 is after t_2 and after t_1 . This illustrates the fact that the relation *before* (and its converse *after*) is transitive. On the other hand, an instant on Sirius located as occurring there at a time between t_1 and t_3 (*e.g.*, my stellar correspondent's anxiety as to whether his message will be returned) is neither *before* nor *after* the instant at which I receive his message. For in order that a message should pass between such an instant and the instant t_2 it would need to travel with a velocity greater than that of light, which is impossible. Thus the situation which is the basis of the paradox of the relativity of simultaneity reappears in Dr. Robb's system in the non-connexivity of the relation *before*.

Any relation which is transitive and asymmetric but not connexive is said to generate a conical order; emphasis on this type of order is a dominating

feature of Dr. Robb's treatment. Not the least of the virtues of this emphasis is the fact that conical order, as the name suggests, can easily be exemplified by a three-dimensional model consisting of a set of cones of exactly the same shape, terminated at the vertex, and having all their axes parallel. If every point in space is made the vertex of one of such a family of cones, the relation between a vertex and any other point within or on the surface of the cone of which it is a vertex will have the formal properties of the relation *before*. While the existence of such a model is irrelevant to the deductive accuracy of a geometry, it has great heuristic value in facilitating the assimilation of a complex theory and may thus promote further researches. The model in question has become an indispensable instrument to the intuitive understanding of the theory of relativity.

This book will remain a classical example of the power and beauty of mathematical analysis.

M. BLACK.

Hume's Theory of the Understanding. By RALPH W. CHURCH, D.Phil. (Oxon.), Assistant Professor of Philosophy in Cornell University. London: George Allen & Unwin, 1935. Pp. 238. 7s. 6d.

DR. CHURCH, having dealt with Malebranche in his admirable volume, *A Study in the Philosophy of Malebranche*, now turns to Hume, between whose teaching and that of Malebranche there are interesting and curious points of contact. I cannot, however, but feel that Dr. Church has been much less happy in the programme he has set himself in this volume. Its brief compass scarcely allows of adequate discussion of the many controversial issues which he raises—the more so as he employs a terminology very different from Hume's own, and it is often difficult to determine whether he is commenting critically upon Hume or is intending merely to expound him. Unfortunately, also, Dr. Church's seven-page Introduction, instead of serving to initiate the reader into the plan and purposes of the volume, is little else than a summary of the subsequent argument, and instead of assisting and encouraging the reader, taxes his resources in a quite unreasonable manner. How, for instance, is the reader either to agree to, or to dissent from, the following statements? "On Hume's theory of abstract ideas 'resemblance' is a term verbal referring (in virtue of habits of association) to any case of a qualitative identity numerically distributed. And, since Hume does not use the term resemblance to name a form distinguishable from intrinsically resembling qualities, his denial of abstract general ideas is not inconsistent with his conception of resemblance as a philosophical relation" (pp. 12-13). So much turns on the expressions 'a term verbal referring to' and 'qualitative identity'. Even more difficult is another passage, no less central to Dr. Church's main thesis. Having stated that the natural relation of cause and effect "differs from those of resemblance and contiguity in being expectant or transitive in intent", he proceeds to speak as if the term 'expectant' were equivalent to 'transitive in intent', and as if both expressions were equivalent to temporal transition. "The true idea of the self, Hume says, is that of a system of perceptions related by cause and effect. The self, then, is a system of habits of causal inference, which differ from habits of belief in independent existence in that they alone are expectant or transitive. Thus, within experience, those perceptions which are felt as wholly actual and self-comprised are to be distinguished from those which are

expectant; and those of the former kind will be appearances of perceived bodies, while the latter are the habitual constituents of a self which is at once mnemonic and expectant" (p. 16). I have found it difficult, even with the aid of the subsequent argument, to decide how Dr. Church intends these statements to be taken; and I also have difficulty in translating them back into any terms actually used by Hume himself.

When we hasten on to chapter I, in the hope of finding these questions treated at length, our hopes are only very partially fulfilled. The issues raised are indeed those that are fundamental, but just for this reason they call for more elaborate discussion than Dr. Church anywhere allows to them. Thus in his references to Hume's use of the term 'representative' as applied to ideas, he speaks (p. 22) of Hume as teaching that "impressions are not expressions, for they refer to nothing beyond themselves", and would seem to be taking Hume as maintaining that, in contrast to impressions, ideas are not merely copies of impressions but are also *consciously* representative of them. Dr. Church even goes so far as to state (p. 28) that Hume "assumes that in being copies, ideas know their originals". But it is not made clear whether this is to be regarded as an inconsistency in Hume or as being involved in his avowed teaching from the start. If Dr. Church intends to suggest the latter view, he ought to have cited the passage or passages on which this interpretation of Hume is based, and to have reckoned with it more than he does in other connections. I cannot myself agree that Hume ever made any such assumption. For Hume, as for Locke, the essence both of mind and of matter is unknown; and in his theory of knowledge, as in his theory of morals, he assumes as ultimate the fact of apprehension, alike as regards impressions, ideas, and 'philosophical' relations. The mechanism of association serves only for the enlargement and ordering of what are thus given, not for any transition *in intent* whereby the mind is carried either to or beyond them. This side of Hume's teaching may, as Hume admits in the Appendix to the *Treatise*, be seemingly incompatible with his own more distinctive doctrines; but he does not attempt to dispense with it, merely on that account. Yet that, so far as I have been able to follow Dr. Church's argument, is what Dr. Church would have us do.

The difficulties multiply when Dr. Church advances in his penultimate chapter to the question, how Hume distinguishes between the self and perceived bodies. The concluding chapter is on 'Knowledge and Belief'.

NORMAN KEMP SMITH.

Bernard Bosanquet's Philosophy of the State. By BERTIL PFANNENSTILL.
Lund: C. W. K. Gleerup, 1936. Pp. iv + 324. 10 kr.

ONCE again a Swedish author has written a book upon English idealism. This is the second work upon our idealists, and the third upon aspects of British philosophy, to appear from Lund within a few years. Like its predecessors it bears indubitable testimony to painstaking and able research.

The author distinguishes three methods in political theory, *viz.*, the juridico-normative, the sociological, and the ethico-normative or philosophical. To this last, of course, Bosanquet adheres. Mr. Pfannenstill argues, I think justly, that the two first methods, which purport to be special sciences, are not really self-sufficient, but disclose under criticism their need of a philosophical theory. In his further step, however, he is less easy to follow, for he maintains that the function of political philosophy "in its true sense" is, "to give us a totality (*sic*) view of value and

reality" (p. 32). Does not this amount to a confusion of the plain position that any philosophical study is an unlimited one, and the quite untenable position that every philosophical study must have immediately in view the most general and unlimited object, namely all that is real and valuable, or what the idealists call 'the Whole'?

By far the greater part of this book is concerned with the metaphysics and logic of Bosanquet, and of the connections therewith of his political philosophy. This certainly follows from the definition just cited; and, in any case, where a thinker of Bosanquet's type is dealt with this method naturally suggests itself. Nevertheless it could be contended that this constant reference to the metaphysics for grounds supporting the political doctrines might succeed rather in sowing suspicion of the former than in fortifying the latter. Whether or not one agrees with them, Bosanquet's political doctrines have a good deal more in their favour than has much of his metaphysics and logic. Hence it seems to me a little unfortunate to burden them unduly with the subtleties and 'many sided' circumlocutions of his initially hopeless attempt to make sense of the finite world in terms of his timeless Absolute, or of freedom and individuality in terms of his supra-relational Whole.

Mr. Pfannenstill's historical survey brings out clearly the depth of Bosanquet's debt to Rousseau, though perhaps it does not sufficiently mark their profound difference of spirit. This section (though, from the point of view of the unity and aim of the book it may be somewhat over-weighted) is the more interesting in that many of its citations are from thinkers whom we have small opportunity of reading.

As to the political theory itself, the point most likely to stick in the English reader's throat when he turns to it fresh from his daily newspaper is Bosanquet's belief (candidly insisted upon by the expositor) that the Nation-State is "the highest instance of ethical value". Or even more strongly, "For him the state is 'the guardian of a whole moral world,' but does not itself act within a moral world" (p. 293). It must be remarked that Mr. Pfannenstill is aware of a difficulty here, but neither in this case nor elsewhere does he permit criticism to be pressed home. He quotes, in another connection, from Prof. Laski; "the will of the average English Banker has no identifiable relation with the will of a South Wales Communist . . ." (p. 223), but he does not make clear how crucial this example is for a view of the state founded upon a 'general will'. No sounding of familiar trumpets within the camp can meet this kind of attack from without the walls. The reply given is, that "the unity (of the state) is not absolutely frictionless, but is a working system, the different parts of which interweave on account of their activity". "The State and society is a progressive integration, a unifying whole" (p. 224). Can such optimism survive the 'interweaving' and 'progressive integration' now proceeding in Spain? Perhaps, since it has withstood so much. Yet it would be unfair to lay this as a charge against Mr. Pfannenstill, who clearly takes it as his task to remain, as far as possible, within the categories of his subject.

Bosanquet's theory of the state is well enough known to make further detailed review needless. It will suffice, therefore, to repeat in conclusion that as a piece of sustained sympathetic exposition this book reflects great credit upon its author. If his pages make an acknowledged opponent of much of Bosanquet's thought bristle with objections, this is but a testimony to the fairness and fullness of his work. And to an advocate of the sort of view dealt with it can only appear masterly.

RALPH E. STEDMAN.

On the Bringing Up of Children. By Five Psycho-analysts, ed. by JOHN RICKMAN, M.D. London: Kegan Paul, 1936. Pp. xvi + 237, 6s.

THIS little book has merits too numerous to detail. It is short, readable, and full of information of a kind so sadly needed. It would indeed serve as an excellent preliminary to the deeper study of psycho-analysis.

It is the first psycho-analytic contribution of a positive nature to education. So far psycho-analysis has offered but negative advice. The delay has, no doubt, been justified. Well-meaning psychotherapists of little scientific training very likely do more harm than good by rushing into print. Here, however, we have the most up-to-date views of the most highly trained psychologists. For psycho-analysts of to-day have to undergo a long training before qualifying; and additionally long if they wish to specialise in children. It is of interest that child psycho-analysts are nearly all women—all the contributors to this volume are.

Naturally any psycho-analytic work will emphasise the importance of fantasies, how a child will manufacture his own dreads, and so on. Thus constipation and allied difficulties are shown to be frequently connected with the child's fear that he has swallowed up his mother—a fear so strongly manufactured by the unconscious that it is not even dispelled by the evidence of his eyes, that his mother is there before him quite plainly not eaten up. Questions like this, so frequently regarded as fantastic, are very simply explained: the "fantastic" side of the subject is largely eliminated. Thus the authors stress the internal powers of the mind. It is not, as "environmental" psychologists would have us believe, "a piece of inactive plastic material" to be "conditioned" as we think fit. "The real qualities of the parents make an enormous difference to the child's development, and the effect of their behaviour is never to be underestimated. And yet it is not in itself the whole problem. The child's inner psychic life has its own contribution to make to the complexity of his relation with his parents" (p. 169).

The moral of all this is that parents must not be worried if their offspring does not develop regularity of habits as laid down by text-books; and that children should not be drilled in habit so early as is usually done. Details of what may be expected by certain ages are carefully given.

On the other hand, the authors attach due weight to environment. A great deal can be done to mitigate childhood disturbances. It is not possible for parents to possess a working knowledge of psycho-analysis. But they can learn something of the child's *general attitude* towards life; and without a knowledge of this, modifications of environment cannot hope to be effective. The authors discuss in detail the kind of changes that are practical.

Accordingly a great part of the book is devoted to the child's experiences; and of these a very vivid description is given. "When he is hungry, he does not know that food is coming to him presently. He cannot realise that his mother has her eye on the clock and is making preparations even while he waits in frustrated pain. . . . Whether it be satisfaction or loss, pain or pleasure, *the infant's experience is absolute. He does not even know that his mother is there in the world as a source of future satisfaction*" (pp. 170-171, *my italics*). In general what we have to bear in mind is that if a child is behaving in some extreme or unusual way, he is probably demanding help and sympathy: he merits neither encouragement or reproof. The difficulty is that tactful treatment depends upon the parents' showing a natural insight into the situation or trouble. Upon this, too, will depend

the success of all practical planning. That is to say it is no use for parents to plan their child's upbringing if their own unconscious emotional attitude makes that upbringing impossible.

"The remaining function of the good parent is then to know when to call in outside help. Just as the wise mother knows when to send for the doctor and the nurse . . . so it is highly desirable that she should have sufficient wisdom and love to know when to call for the help of the psychoanalyst. And this may sometimes occur even with children of wise and healthy parents. We have seen that much more can be done than is generally realised at present to lessen the probability of such serious crises in development. . . . In those cases where the child has become ill or unhappy *in spite of* sense and consideration on the part of the parent, it is very plain that he would have been very much worse if the parents had been inconsiderate or cruel. . . . We have, therefore, to recognise that with *any* child there may come a situation in which the special technical help of analytical treatment is required, to guard against major breakdowns and failures in later life" (p. 225).

One or two remarks before finishing. It will be noted that the entire book is given up to the upbringing of babies! In other words the smoothness or difficulty of later life is inexorably fixed before the latency period. None the less parents of older children will find much to read that may be of value to them. I have tried in this review to describe the aim and attitude of this important book, and have perhaps given little or no idea of the excellent discussion of details which concern every mother or nurse. Lastly philosophers may be interested in an implication of the work, that there is much *empirical* evidence against Behaviourism.

J. O. WISDOM.

L'Esperimento nella scienza, nella filosofia, nella religione. By ANTONIO ALIOTTA. Francesco Perella, Napoli. 1936. Pp. 102. L10.

PROF. ALIOTTA describes his philosophic attitude as that of a radical experimentalism and declares himself the sworn foe of everything abstract, *a priori*, static, rationalistic, passive, and intellectualistic, and the champion of life, activity and movement. He is, in consequence, in general sympathy with modern tendencies in philosophy and science, with Bergsonism, pragmatism, relativism and the new operational interpretation of physics. Thus he fully accepts Heisenberg's principle of indeterminacy and takes it as meaning that the object cannot be abstracted from the subject, that experiment is not a passive reproduction of a phenomenon but an operation upon it, which implies a point of view we have chosen and a co-ordination of our human action with the other agents in the world of our experience (pp. 87-8). There can be little doubt that this broadly philosophic attitude is the right one to assume towards the present predicament of physics. It may well be admitted that means may some day be found to overcome any merely experimental difficulty in a science. But what Heisenberg's Principle calls in question is not merely an experimental difficulty. It is the fundamental assumption of scientific method that the personality and the activities of the observer are irrelevant to the facts observed, and may safely be abstracted from. Now this was a methodological principle which worked well enough, until recently, for the purposes of physics, although it could never claim universal validity. It was always known that some reals, *e.g.*, living creatures, were *not* indifferent to the observations made upon them and to the actions of those who

observed them. It now turns out that this principle does *not* apply to the finer observations of physics either. Hence the principle of the indifference of the object to the procedure of observation sinks logically to the status of a methodological *fiction*, and in this status it will remain, even if the ingenuity of physicists should surmount the experimental difficulties which have led them to enunciate the principle of Heisenberg. For the rest it should be noted that Prof. Aliotta has a happy knack of illustrating his argument by apt references to the history of philosophy, and that his expositions not infrequently rise to eloquence.

F. C. S. SCHILLER.

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VII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY. xxxiii. (1936), 6. **H. Reichenbach.** 'Logistic Empiricism and the Present State of its Problems.' ["The evolution of science in the last century may be regarded as a continuous process of disintegration of the Kantian synthetic *a priori*." First, the concept of probability was shown to be logically prior to that of causality. The principle of induction, however, remained an unsolved problem. Wittgenstein and Carnap attacked the problem of meaning, defining logic as "the totality of tautologies", and a tautology as "a combination of elementary propositions which remains true for all possible truth-values of the elementary propositions". Every kind of metaphysics thus becomes 'meaningless'. Further, it appeared that meaning is no real entity but reducible to a relation between physical objects. So "logistic positivism became logistic materialism". However, "the predictive character of science" could not be justified in this way. So Reichenbach appeals to his theory of probability, according to which no proposition about the future claims to be certainly true and the truth-values "are replaced by a continuous scale of probability". This leads to an answer also to the problem of induction: we get no demonstration indeed that induction *must* lead to success, but it is our best chance.] xxxiii., 7. **C. J. Herrick.** 'Is Truth a Value?' [Attacks M. H. Moore's argument in xxxii., 20., that truth cannot be a value if it is defined as a function of interest, because the truth of a proposition is independent of our interest in its truth. This is "to cut ourselves off from any hope of an effective articulation of propositions of either truth or value into the vital problems with which both science and philosophy are properly concerned". It would "devitalize science and sterilize philosophy", and if Aristotelian logic sanctions such procedures, the sooner it is supplanted by another the better.] **R. Rothman.** 'Value and Intelligence.' [Defends Dewey's theory of values against a criticism by J. R. Reid in xxxii., 13.] xxxiii., 8. **S. P. Lamprecht.** 'Philosophy of History.' [Holds that "all natural events should be treated in terms both of causality and contingency" and that so "we are not inevitably headed towards glory or ruin or any other predestined fate. What will come will be in large part a function of our courage and our understanding."] **R. Kroner.** 'Philosophy of Life and Philosophy of History.' ["Historical life is the life that sees with an inward eye, and acts according to an inner vocation; thus it is a life of spiritual necessity. Spiritual necessity is what we call freedom", etc.] **P. Hughes.** 'Current Philosophical Problems.' [Great philosophic literature keeps in touch with earth, but the activities of professors don't.] xxxiii., 9. **A. C. Benjamin.** 'The Concept of the Variable-Given.' [Taking the variable-given as including the hypothetical, the inferred, and even the fictional and the non-existent, we must agree how the various givens are to be inferred from one another. The variable-given seems to be implied in all science, which "never attempts to set up the limits of the knowable". It has also the advantages that it lumps together all suppositional entities into a single class, brings out the relativity of the distinction between invention and

discovery, and shows the difference between rationalism and empiricism to be only one of degree.] **C. J. Ducasse.** 'Verification, Verifiability, and Meaningfulness.' [An assertion being something which answers or forestalls a question, and a question being about something not itself in question but given, its 'truth' is tested by a specified verificatory procedure. But this may not be actually carried out, though it seems possible theoretically. How then shall we be sure that it is even theoretically possible? The answer suggested is that the theoretic procedure must be sufficiently like, and relevant to, procedures already recognised.]

E. W. Hall. 'Of what use is Metaphysics?' [After rejecting the views of G. E. Moore, Wittgenstein and S. C. Pepper, the author declares that metaphysics "does deal with the class of all existents", attempting to abstract "the general form of existence which constitutes the uniqueness of any given *qua* existent".] xxxiii., 10. **J. Dewey.** 'Characteristics and Characters: Kinds and Classes.' ["If propositions about singulars, characteristics and kinds, and propositions about universals, characters and classes bear a necessary logical relationship to each other, then logical theory must recognize both types of propositions, and is under obligation to frame a coherent theory of their relation. The confusion of the two types, exemplified in constant use, in current texts, of generic and universal propositions as being of the same form, both begs the question at issue and obscures what is probably the most fundamental problem of logical theory."] **H. G. Alexander.** 'Linguistic Morphology in Relation to Thinking.' [Illustrates the differences in the morphology of languages from the various ways in which time-relations are expressed in a number of American Indian languages, and so suggests what would probably be a very fruitful inquiry into the development of 'categories'.]

P. A. Bertocci. 'The Authority of Ethical Ideals.' ["At any moment in the development of ideals, whatever ideal is considered the best by the critical intellect evokes that ultimate desire-transcending experience which we designate by 'ought'."] xxxiii., 11. **J. Dewey.** 'What Are Universals?' [Discusses the question in the traditional way, in terms of realism, nominalism and conceptualism, but adds that "every universal is a formulation of an operation to be performed", and that "a physical or existential law" can never become more than probable because its proof always involves an affirmation of the consequent. Holds further (1) that by taking the relation of universals to individuals as logical rather than ontological the old Platonic difficulty of participation may be avoided; (2) that since "the universal is determined with reference to possible applicability", it does not dwell in a separate realm "independent of *any and all* application"; (3) that the universal is ideal in the sense of being normative or prescriptive; (4) that it is subject to revision. "The idea that the universals are already, in and of themselves, Eternal Objects, Essences, etc., and that by trial and error we finally hit upon the particular one which is applicable to particular cases is gratuitous." In short, Dewey seems to think that more is to be gained by patching up the Platonic theory of universals than the Aristotelian.] **T. A. Goudge.** 'Further Reflections on Peirce's Doctrine of the Given.' [Continues the discussion with Dewey (*cf.* xxxii., 19 and 25), but does not admit that Peirce's various dicta can be rendered consistent.] **J. Somerville.** 'The Social Ideas of the Wiener Kreis's International Congress.' [Reflects on the Paris Congress 1935, announces that the 'logical positivists' desire to repudiate Comte and to be henceforth known as 'scientific' or 'logical' empiricists, emphasises that their doctrine is still fluid and changing and that they seem to be gradually discovering the pragmatism

of James and Dewey. The writer hopes that the movement may do something to mitigate the narrowness of American sociology.] xxxiii., 12. **D. S. Miller.** 'James's Philosophical Development; Prof. Perry's Biography.' [Not so much a review of Perry's book as a criticism of James, chiefly on account of his indeterminism.] **R. E. Fitch.** 'The Two Methods of Ethics.' [The dialectical, and the experimental. Proof in the latter is progressive, in the former "circular—a perennial begging of the question".] **J. Dewey.** 'One Current Religious Problem'. [Apropos of P. Hughes (xxxiii. 8) doubts whether there is only *one* such problem.] xxxiii., 13. **A. G. A. Balz.** 'The Metaphysical Infidelities of Modern Psychology.' [A highly amusing article, arguing that "psychology, in the modern world, is the result of an impossible effort to be, at one and the same time, Aristotelian and Cartesian". Were it faithful to Cartesian dualism, it would not attempt to correlate 'matter-events' with 'soul-events'. Hence "psychophysical parallelism, interactionism, double-aspect theories—all of these are disguises for a surreptitious theology", needed because "psychologists without laboratories are dreadfully unhappy, and without theology they cannot ask administrative heads for rooms and gadgets with which to amuse themselves. . . . They affirmed that if they could only be given some pickled brains and some electrical machinery, they could tell what goes on in the soul, because what goes on there has an accompanying going-on somewhere else. . . . A simple faith in the competence of rational theology have these psychologists. And they did get their laboratories, together with monkeys and white rats."] Abstracts of Papers read at the 37th Annual Meeting of the Western Division of the American Philosophical Association, State University of Iowa, April, 1936. xxxiii. 14. **W. Jewell-Lapan.** 'Perception and Reality.' [Thinks that "the difficulties of the realist have arisen from not distinguishing between perception and knowledge". It is 'meaningless' to assert that we perceive the world as it really is, and false that perception is knowledge. If sensations and perceptions are taken as processes and not existences, it is irrelevant to ask whether they are 'subjective' or 'objective'.] **C. Barrett.** 'In what Sense are Values Objective?' ["In the sense that such things as *life* are objective." As leading up to this answer it is argued (1) that a valuing organism could not evolve 'naturally' in a valueless universe, (2) that a world-order devoid of value could not either stimulate or satisfy the universal human demand for objectification of values, (3) that on no naturalistic theory are the permanence, contrasts, imperatives, and communicability of value-experiences explicable.] **M. W. Hess.** 'A Note on Criteria.' [Comment on E. Vivas, xxxiii., 3.]

REVUE DE MÉTAPHYSIQUE ET DE MORALE. 42^e Année. No. 1. January, 1935. **L. BRUNSCHWIG,** *Religion et Philosophie.* [Address delivered before the 8th Congress of Philosophy at Prague. Begins by distinguishing two philosophical attitudes towards religion, *viz.*, the "idealist", which accepts religion as the revelation of a supernatural reality, escaping ordinary methods of verification, and the "positivist", which treats religion as a "human function", like science and morals, subject to the same standards of judgment, a chapter in the "natural history" of the human mind. Philosophy appeals to "reason": what can reason do with religion? There is the way of dogmatism, which erects a system on arbitrary premises. But the recognition of the arbitrariness of alternative premises leads to scepticism. Both dogmatism and scepticism have been exploited in the interests of religion. But when religion has been defended

in terms of a particular philosophical system, itself religiously neutral, or "profane", religion has become exposed to the attacks to which the system itself has been open. On the other hand, when defenders of religion have sought to argue that, because different dogmatic systems cancel each other out, therefore religion can stand without philosophical support, they invite the retort that, similarly, the differences between religions lead to religious scepticism. By contrast, Brunschvicg advocates a third way in which reason can deal with religion, *viz.*, by re-interpreting it so as to give it a rational content; "rational", in the same sense in which science is rational. There can be but a single truth, and religious truth is simply truth. Reason ("the Word", Logos) is God. What we need is a transformation "of religion of Nature *sublimated* into religion of Nature *surmounted*". Theism, in its historic forms, has been merely religion of Nature sublimated into an anthropomorphic God with an anthropocentric interest. Religion of Nature surmounted cultivates a disinterested objectivity of outlook, renouncing all privileged position for man on earth. This objectivity has its scientific form in the search for factual truth, and its moral form in emancipation from exclusive individual or national interests and in the service of a universal altruism. Thus science and morality, no longer antagonistic, lead on to the reign of reason both in theory and in practice, which will be the true reign of God. Brunschvicg concludes with a plea for international peace and co-operation in the name of the one true religion of reason, underlying the many diverse historic faiths. This will be Christianity perfected: "it is one and the same thing to learn to think and to learn to love".] **L. Dugas.** *La Théorie de la Mémoire affective chez Maine de Biran.* [The problem is: can we *at will* recall feelings, emotions, the sentimental colouring of past events, as distinct from recalling these events themselves? This question becomes partly: How to distinguish a feeling recalled from a feeling imagined or even a feeling re-felt (*i.e.* the present experience of a feeling more or less of the same sort as the feeling experienced in the past)? Partly, again, the question is: What means, *i.e.*, what "signs", have we by which to effect the recall of a feeling? Can words play that part? The language of feelings is other than the language of "ideas", and the best language for the former is music, thus showing that ideas and feelings are, in principle, separate and distinct. Signs may represent; they may evoke; they may excite. The signs for feelings tend to excite, rather than to represent or evoke, thus substituting imagination or fresh present feeling for genuine recall. Yet, as the example of Proust's works shows, the skilful use of ideas and images may evoke feelings experienced in the past and thus mediate that memory of feelings, the nature and conditions of which Maine de Biran was trying to analyse.] **J. Devolvé.** *Rôle esthétique de la Récurrence (premier article).* [I find the word *réurrence*, as used by the author, untranslatable by any single English word. It means at once emancipation from the utilitarian, practical, interested attitude in the apprehension of an object, and the achievement of a disinterested attitude in which the percipient experiences a sense of the fundamental unity of Reality, conveyed to him by the universal significance which the object, æsthetically apprehended, reveals to him. The word "recurrence" is used because the author regards the enjoyment of this unity of Reality as characteristic of the fundamental immediacy of experience which is broken up by the analysing and relating work of thought, largely under the pressure of practical urgencies, and to which, in æsthetic experience, we return or recur. One of the author's examples may serve to illustrate what he means by *réurrence*. Baudelaire's poem, *La Charogne*, has for its subject a repulsive putrefying carcass, which in practical life evokes only an emotion of

aversion and a reaction of avoidance, but which for the poet's vision retains "*la forme et l'essence divine*" of the loves it had experienced, thus being a symbol at once of death as the destiny of all living things and of love as the glory of all life. The first stage of recurrence is this emotional purification and the recovery of naive, child-like sympathy of apprehension; the second, "creative imagination", is the elaboration in appropriate imagery of the universals which have thus become visible in the particular, and of the emotional mood appropriate to them. At this stage the work of art is born. The essential function of *réurrence* is "to open or re-establish the communication between the obscure unity of Reality and the rational clarity of consciousness". Hence, *réurrence* is a form of rational activity, being thus the highest manifestation (because on the plane of rational mind) of an æsthetic activity which runs through all Nature and which can be traced through all the levels of evolution, from the unconscious physical plane upwards.] *Études Critiques*. **L. Weber.** *La Pensée et le Mouvement, par Henri Bergson.* [A critical review of Bergson's articles collected under this title. More valuable than most of these reviews, in that, after a careful summary of Bergson's views, it formulates some seven points of criticism of which the most important are: a challenge to Bergson's view that the "intellect" models itself upon "matter" which is its proper object; that the practice of intuition, as the only proper method of philosophy, can be learnt by everyone and that its results are the same for different practitioners; that recent developments in physical and biological theory, in so far as they have been reached by the ordinary methods of science and not by intuition, can be adduced by Bergson as confirmations of his own conclusions, etc. The review concludes with some interesting observations on Bergson's recent arguments for individual survival of death, and the affinity of his speculations in this connection with Spiritualism and Theosophy.] **M. Gueroult.** *L'Odyssée de la Conscience dans la dernière Philosophie de Schelling, d'après M. Jankelevitch.* [Challenges, as arbitrary and far-fetched, Jankelevitch's attempt to establish a close affinity between the thought of Bergson and that of Schelling in his last phase, and counters by exhibiting in detail the affiliation of Schelling's thought with the movements of thought in his own time. The final verdict is crushing: Jankelevitch's study of Schelling differs from a serious contribution to the History of Philosophy as much as a novelist's romance of the life of some famous person differs from a scientific biography.] *Questions Pratiques*. **A. Ouy.** *Le Scandale et la Faute.* [Defines "scandal" as an action which evokes indignation and surprise by being a spectacular breach of standards or conventions binding on a class or group, by a member of such class or group who had been trusted to uphold and respect them. Such a breach reflects on the whole class: the failure of one member argues the potential liability to failure of all. The act of one lowers the prestige of all: it brings shame and dishonour on the whole group. Hence, a defence reaction on the part of the group: an attempt at all costs (even at the price of letting the offender escape punishment) to avoid publicity, to hush up the scandal, to preserve the good name of the group. But no class has a prerogative of virtue or moral infallibility. Far better to acknowledge and punish a misdeed, even at the expense of a scandal, than shelter a moral lapse behind hypocrisy. The more highly-placed the miscreant, the severer ought to be the punishment. In a democracy, above all, no ill-deeds of high officials should be covered up.] *Variétés*. **D. Parodi.** *Les Congrès de Prague et de Cracovie.* [A survey of the papers read and the chief discussions at the eighth International Congress of Philosophy and the Sixth International Congress of Moral Education.] **J. Cavailles.** *L'École de Vienne au*

Congrès de Prague. [An excellent account of the development of the theories of the Vienna School (Carnap, Schlick, Reichenbach, and others) out of Wittgenstein's *Tractatus*, and of the main criticisms and comments provoked by it at the Prague Congress.] *Supplement.* New Books, French and Foreign. Periodicals.

No. 2. April, 1935. **Ed. Le Roy.** *Ce que la Microphysique apporte ou suggère à la Philosophie (premier article).* [Modern Physics has compelled the physicist to re-examine his concepts which is a task essentially philosophical, whilst philosophers cannot avoid scrutinising their own synoptic conclusions in the light of the contemporary evolution in physical theories. This interaction of Physics and Philosophy appears mainly in respect of four problems, the first two of which are dealt with in the present instalment. (1) Physics, thanks to modern astronomical researches into far-distant stars, on the one hand, and, on the other, to the exploration of the constitution of the atom, has really become a science on *three* planes or levels: the "middle" level on which it deals mainly with the physical objects presented to ordinary perception on the surface of the earth; the level on which it deals with the infinitely large and infinitely distant; and the level of the infinitely small. These three planes of physical science reflect a corresponding stratification of the universe, and when we reflect on the widely different techniques of observation and measurement on these three planes, we become aware of a "pluralism" which compels us to ask, at once, whether the concepts developed on the middle plane remain adequate when transferred to one of the other planes; and how the unity of science, corresponding to the unity of the universe, can be re-established. Perhaps each plane requires its "autonomous" concepts, and, if so, the task suggests itself of devising some inclusive concept which will stand to the several concepts of the three planes in the same relation as a variable to three distinct values. (2) The second section of this first instalment of the article pursues this line of thought into detail by showing how the concepts of time, of three-dimensional space, and, in general, the apparatus of Newtonian Physics, all of which were developed on the "middle" plane of Physics, break down, or become inadequate, when used on the planes of the very large or the very small. We must, therefore, recognise the limited validity of the concepts of classical Physics. This section contains an exceedingly interesting discussion of how the concept of three-dimensional space is developed from our visual and tactual space-perception and our practical behaviour in response to visual and tactual stimuli.] **R. Berthelot** *L'Astrobiologie et la Pensée de l'Asie : essai sur les origines des sciences et des théories morales (suite).* [The sixth instalment of a series of articles begun in 1932. The sub-title of this instalment is: *La troisième Asie : Le monothéisme Islamique et Judaïque dans ses rapports avec l'Astrobiologie.* Its general thesis is that monotheism, like astrobiology, is an intermediate stage in the development from savage beliefs to modern science. But Islamic monotheism follows a divergent path. Astrobiology and it are "like hostile brothers", opposed, yet complementary: astrobiology emphasising necessary laws; Islamic monotheism, the incalculable will and power of God, and actually adopting atomism for the purpose of its polemic against necessary law.] **J. Devolvé.** *Rôle esthétique de la Réurrence (suite et fin).* [Continues the article begun in the previous number by applying the concept of recurrence, as there developed, to a number of standing problems of æsthetic theory, such as the relation of inborn artistic endowment to cultural influences; the nature of the æsthetic "transposition", i.e., the imagery used by the artist to express his æsthetic vision; the question of progress in Art; the motif; the

artistic model and schools of Art (like realism, symbolism, etc.). The article concludes with two final illustrations of the concept of recurrence, viz., Moussorgsky's *Boris Goudounow*, and Racine's *Phèdre*. Beauty is said to be "the explosion in a particular form of universal being". Recurrence is described as "that highest activity of the Spirit by which it links the effects of its work of representation with the Eternal Being". In detail: Every young human being is a poet—not yet divorced from the direct apprehension, and participation in the joy of sheer being, of Nature, by the pressure of economic needs and the sophistication of intellectual analysis. The young human enjoys life as a bird enjoys itself in song, both joys being utterances of the joy in its own being which characterises Reality as such, and which it is the function of "recurrence" to re-capture on the level of consciousness. The artist keeps and expresses this child-like power to enjoy the world, where others lose it and can only find it again through his interpretation of it in his works of art. Again, there can be no "progress" in the essence of this æsthetic enjoyment, which is perfect in all its manifestations. But there is "progress" in that this æsthetic enjoyment is easier to cultivate in simple cultures, and that it is harder to recapture and communicate it against the adverse pull, and yet also by means of the greater richness, of the resources of an advanced civilisation.] *Notes.*

B. Croce. *Action, Succès et Jugement dans le 'Vom Kriege' de Clausewitz.* [This famous treatise on the Art of War raises many points of general philosophical interest, e.g., on the relation of theory to practice; on rules and genius ("rules are not opposed to genius . . . the true rule is precisely the way in which the genius acts"); on the historical judgments on the errors of great commanders: can the critic ever put himself adequately at the point of view of the criticised, so as to see the situation with his eyes? The whole article is a tribute to the philosophical quality of Clausewitz's mind.] *Études Critiques.*

H. Dreyfus-Le Foyer. "*Le Parallélisme logico-grammatical*", par Ch. Serrus. [Good analysis and criticism of Serrus' book. An incidental remark, attributed to Duhem, is interesting, viz., that French physicists "seek order through abstraction", whereas English physicists "are indifferent to the logical development of theories, and incapable of understanding without mechanical models and concrete representations".] *Questions Pratiques.*

G. Poyer. *Population et Progrès.* [Valuable criticism of Landry's book on *La Révolution démographique*, in which he calculates that, at the present rate of the restriction of births, the population of Europe will soon diminish and in five generations shrink to so low a level, that the very survival of Western civilisation will be in danger from lack of human beings to sustain it. Poyer argues forcibly against Landry's view that birth-control is simply due to the influence of "rationalisation" and a desire for material "progress" in individual minds. He holds, on the contrary, that large-scale phenomena, like the rapid increase of population in industrialised countries during the nineteenth century, and again the present tendency to birth-restriction in these same countries, must be understood, not solely through their presumed psychological causes operating in the minds of countless individuals taken singly, but through historic and economic causes affecting whole groups and not appreciated in their true character by the mass of individuals at all. E.g., if in the eighteenth century the population of the British Isles increased very slowly, whereas it quadrupled during the nineteenth century in spite of heavy emigration, can we plausibly infer that the nineteenth century British were less rational or less desirous of progress than their eighteenth century ancestors?] *Supplément.* New books, French and Foreign. Periodicals. *Obituary:* Pierre Tisserand.

VIII.—NOTES.

1st June, 1936.

TO THE EDITOR OF "MIND".

SIR :

In his article on "The Meaning of Implication" (MIND, April, 1936), Mr. Daniel J. Bronstein's criticism of my definition of implication is so fraught with confusion and error that I feel obliged to ask your kind permission to allow me to make the following points :¹

(1) From my assertion that every proposition is consistent with itself, Mr. Bronstein infers that "there can be no contradictory propositions" (p. 167). Though it is not my purpose in this letter to defend my views, I wish to point out that my assertion does not imply that there cannot be a proposition p such that it entails r and s , where r is inconsistent with s . Such a proposition, if there be any, might be called "contradictory"; but it does not follow that, as a whole—as one proposition— p would be inconsistent with itself as a whole.

(2) On page 168 he says, "To argue that because the principle of the syllogism can be deduced from $pqEp$, and $pqEp$ does not hold, therefore the principle of the syllogism does not hold, is to commit an obvious fallacy". Indeed it would be, but my paper contains no such argument. It is not fallacious, however, to argue that, if for any propositions p , q , the general form $pqEp$ is unacceptable, a specific case of that form, $(pEr)(rEr)E(pEr)$, is unacceptable.

(3) On the same page he says that I "contend" that the principle of the syllogism holds *only when* $p * q * r$. I asserted that $p * q * r$ is a sufficient condition of that principle, not that it is a necessary condition.

(4) Mr. Bronstein is right in saying that I did not assert $pqEp$ because I wanted to avoid such paradoxes as $p-pEq$. During the six or seven years since I wrote that paper I have changed my mind on that point. I still believe, however, that $p-pEq$ is unacceptable, but that it can be avoided by means other than the rejection of $pqEp$.

(5) On page 169 he concludes that in view of his criticisms my definition of entailment or intensional implication does not successfully analyse the real meaning of implication. I should be the first one to admit that I did not fully exploit the logic of intensional implication, but from this fact, and even if we grant all of Mr. Bronstein's criticisms, it does not follow that the definition I gave is incorrect. In fact, he does not criticise the definition itself, but only assertions in which the defined term occurs. From the proposition that a postulate employing a defined term is unacceptable, it does not follow that the definition of that term is unacceptable. Hence Mr. Bronstein's conclusion is a *non sequitur*.

Surprised, indeed, I am that in his criticism—purely destructive in kind—of my definition of entailment, he does not even give me or the reader the benefit of stating that definition. How, I ask, is a reader intelligently to evaluate a criticism of a definition or analysis if that definition or analysis is not even presented to him? In view of this omission, I beg to state my definition: " p entails (intensionally implies) q " means " p is inconsistent with the proper contradictory of q ".

Respectfully yours,

EVERETT J. NELSON.

University of Washington,
Seattle, Washington.

¹ All his references are to my article, "Intensional Relations", MIND, vol. xxxix. (1930), pp. 440-453.

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MIND

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OF

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EDITED BY

PROF. G. E. MOORE,

WITH THE CO-OPERATION OF PROFS. F. C. BARTLETT AND C. D. BROAD.

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